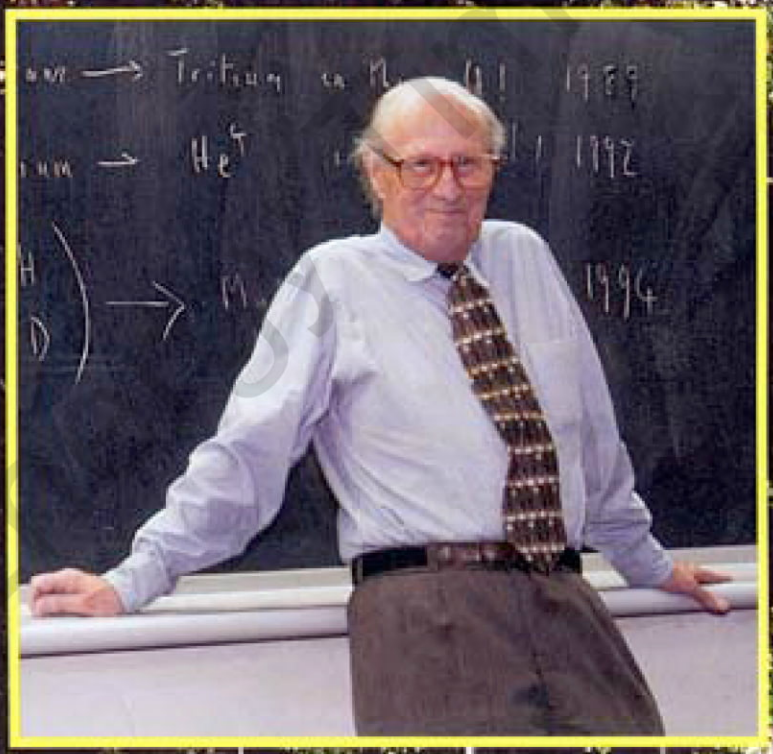
# INFINITE ENERGY™

The Magazine of New Energy Technology
Cold Fusion • New Energy • Renewable Energy
Volume 6 • Issue 32 • 2000

## THE TRIUMPH OF ALCHEMY

Professor John Bockris, Pioneer Cold Fusion Researcher, and the Transmutation Crisis at Texas A & M



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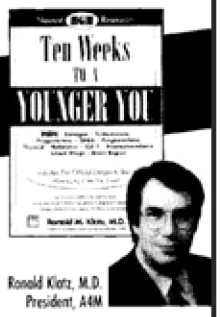


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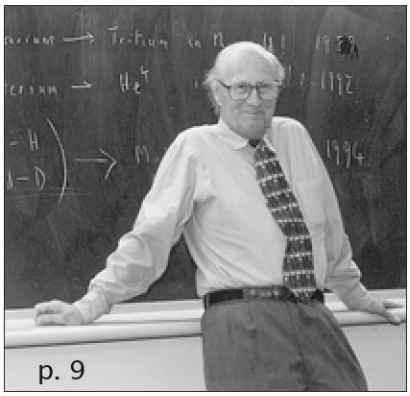


**Professor John Bockris**
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**The Transmutation Crisis at Texas A&M**

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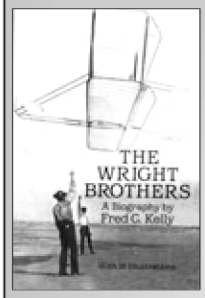


**The Wright Brothers: A Biography**
Fred C. Kelly, 1989 (reprint), Paperback, 368 pp.
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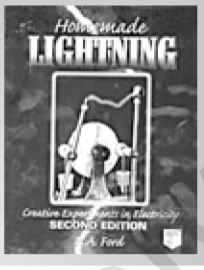


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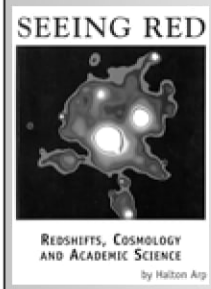


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Halton Arp, 1998,
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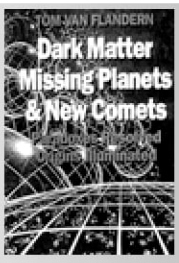


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Tom Van Flandern, North Atlantic Books, 1993, Paperback, 514 pp.
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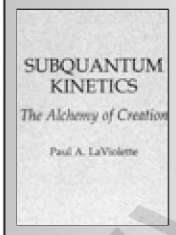


**Subquantum Kinetics: The Alchemy of Creation**
Paul LaViolette, Starlane Publications, 1994,
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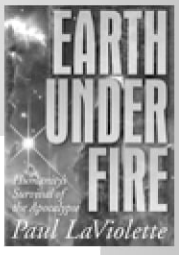


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Paul LaViolette, Starlane Publications, 1997,
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There is a one-of-a-kind Beatles Yellow Submarine page with more complete information than you will find anywhere else on the web, since Dr. Bob Hieronimus is the world's leading expert on this film and has a forthcoming book on the subject to be published later this year. The most visually stimulating new feature is The Hieronimus Art Gallery where samples of Dr. Bob's mural work and other designs from the past three decades are posted. Long before he became a broadcaster, Bob was a highly acclaimed symbolic painter, and much of the research presented on 21st Century Radio is expressed in his paintings which can be read and interpreted on numerous levels. They are all powerful designs in vibrant colors, so even if you don't "get" the message, they are uplifting to behold. Hieronimus & Co. have been big supporters of *Infinite Energy* Magazine and have featured Editor in Chief, Gene Mallove, as a guest over a dozen times, with regular updates every few months.

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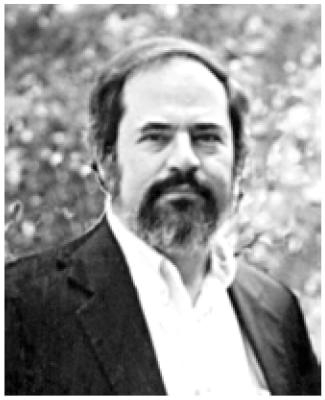
# BREAKING THROUGH

## Anomalies, "Infinite Oil," and Cold Fusion



*by Eugene F. Mallove, Sc.D.*

On the last day of the conference, Dr. Francesco Scaramuzzi, of the official Italian cold fusion program run by ENEA, summarized the Eighth International Conference on Cold Fusion (ICCF8): ". . .Strong confirmation that we have the production of excess heat and helium and their correlation. Yes, there is excess heat and, yes, it is of nuclear origin." He went further: The work on transmutation presented at the conference was an "indication of the nuclear character of the phenomenon." Others followed with similar conclusions at the conference which was held in Lerici, Italy on May 21-26. For the first time at an international cold fusion meeting, a relatively conservative scientist stated publicly what has been obvious to most of us in the field since the early to mid-1990s: cold fusion is real. The anomalies of excess energy and nuclear changes in low-temperature systems confront modern science with a tough choice: Continue to ignore the evidence, or acknowledge what the general scientific community has bitterly disparaged for eleven years. For some time, the choice will be the latter.

Science progresses along diverse pathways. In one direction, it lumbers forward as it compiles and correlates vast quantities of information. When molecular biologists work for years to define the human genome, that is what they are doing. This is one form of so-called "normal science," discussed by the late Thomas Kuhn in *The Structure of Scientific Revolution*s. Another example: astronomers employing advanced observing equipment to capture and assess torrents of multi-spectral electromagnetic images of the heavens. This is like "rock collecting," so to speak—taking extended field trips to obtain data that can be studied for new patterns, as well as extending the catalog of information in a certain area, such that it has utility for other scientific work, *e.g.* determining the emission characteristics of hundreds of unstable isotopes. A frontier science, such as cold fusion, has benefited immensely from the availability of such impressive catalogs that originate in normal science.

Normal science also occurs when scientists define and carry out experiments that will incrementally add to an understanding within a well-defined area, such as when the biochemistry of a previously poorly understood genetic illness is revealed, or when a general mechanism within cell function is elaborated.

There is a role for the "pure" theorist too, who does not do experiments, but who tries to develop the functional and mathematical relationships that lie hidden within mountains of experimental data.

When an experimental or observational anomaly, such as low-energy nuclear reactions, appears to be completely orthogonal to the accepted theoretical framework, it often suffers a period of rejection until it can prove itself in combat. Vested intellectual interests and academic funding interests set up a minefield that the anomaly must pass through. If the phenomenon should have difficulty being scaled up or even in being made repeatable *on demand*, so much the worse for the anomaly. That has been the unfortunate fate of cold fusion so far.

Some anomalies get lucky and have striking, immediate effects on science, such as the serendipitous discovery of X-rays in 1895 and radioactivity in 1896. In geophysics, the anomalies noted by Alfred Wegener in 1912, which implied continental drift, were ridiculed and rejected until the 1960s, when new evidence *and* theory came together in modern plate tectonics. In the field of medicine, anomalies whose acceptance and life-giving has been held back are so numerous that they are more the rule than the exception. The fate of astronomer Halton Arp's observations about quasars and red-shift are so threatening to Big Bang cosmology that Arp continues to be an outcast. This despite solid observational evidence (see the review of his book, *Seeing Red*, *IE* No. 31, p. 32). Attention to anomalies will play a very important role in the energy field, as cold fusion has already proved.

Everyone knows that we pay and suffer much because of growing needs for energy. In robust economies such as those of the United States, Japan, and many European countries, energy is a serious cost consideration, but not nearly as much as in less-developed nations. There seems no end to the interminable fossil fuel age and the threat that the consumption of fossil fuels may someday outpace the needs of the expanding human population. This perceived future shortfall "justifies" the price of fossil fuels being much higher than they should be. If the idea gained currency that the fossil fuel age might come to an end in less than a generation, as cold fusion and other new energy technologies promise, prices would surely fall.

Another possibility is that the supply of hydrocarbon fuels may be far greater than most experts have assumed. Respected physics professor Thomas Gold of Cornell University thinks that terrestrial oil resources are much larger than previously imagined; he says they are hundreds of times what conventional petroleum geologists accept. That's not "infinite oil" on Earth, but far, far more oil than we thought we had. Gold believes that oil is generated within the Earth, *abiogenically*. It is generated chemically under the great internal pressures on the primordial methane gas that he believes permeates the interior. Such bacteria as are found within oil nearer the surface are living on the oil, he says! He provides copious evidence and compelling theory to support this view—see his book *The Deep, Hot Biosphere* (reviewed in *IE* No. 26, p. 51) and a recent interview with him in *Wired* magazine ("Fuels Paradise," July 2000, pp. 160-172). He cites the presence of helium in oil as evidence that oil

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sweeps up this noble gas from greater depths. In Gold's view, by far the greatest mass of life on Earth is deep underground at temperatures above 100°C, though the oil is produced much deeper. (Note also C. Warren Hunt's similar but even more spectacular theories, proposed in parallel, p. 60 of this issue.)

Gold's theories, and the anomalies on which they are based, do not sit well with establishment scientists, but Gold has been right before on many formerly controversial matters, such as neutron stars. What if he is proved right about oil? Enter the emerging fuel cell technologies that employ hydrogen scavenged from petroleum or natural gas. A recent *Science* article about these ("New Tigers in the Fuel Cell Tank," Vol. 288, June 16, 2000, pp. 1955-56) suggests that fuel cell technologies are accelerating toward the marketplace faster than expected. This may mean that fuel cells for transportation and electric power generation could begin to supplant existing power-generation infrastructure, particularly if these technologies have reduced $CO_2$ emissions, as many do, and are adaptable to distributed power-generation.

The confluence of an emerging "infinite oil" and "infinite natural gas" paradigm with advanced fuel cells represents both an opportunity and a threat to emerging new physics energy technologies. If cold fusion and BlackLight Power Corporation's hydrino energy processes (see article on BlackLight in Briefs, p. 45), do not accelerate fast enough into the marketplace, they could be left behind for some time. It will be fascinating to watch how these technologies fare amid the onslaught of fuel cells in a possible era of emerging hydrocarbon abundance and concern about the threat of human-induced global climate change.

Despite the great technical progress evidenced at ICCF8, we must be frank that the commercialization of low-energy nuclear reactions is in a moribund state. Two corporations that had been in the forefront, ENECO of Salt Lake City and CETI of Sarasota, Florida, have both put their problematic excess heat technologies on the back burner. Another company, Lattice Energy, LLC, founded on a proprietary thin-film electrolytic heat process of Prof. George Miley of the University of Illinois, is just getting off the ground. Despite what we have learned of the excellent financial backing for Lattice Energy, young companies in this area have not fared well. We wish the company well, with all our fingers crossed! Other, less well-financed companies continue to struggle for funding and defend themselves against the obstructors at the U.S. Patent Office.

***Editorial continues on page 69***


# New Cold Fusion Book
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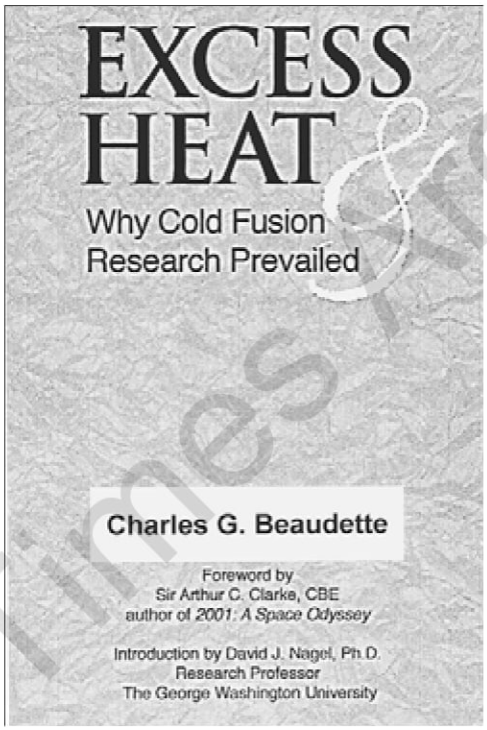


**Foreword by Sir Arthur C. Clarke**
**Introduction by David J. Nagel**

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**—Sir Arthur C. Clarke**


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# LETTERS TO THE EDITOR

**Voodoo Scientist**

Gene Mallove's comments on Robert Park (*IE*, No. 30, pp. 44) were absolutely devastating. It looks like Park was educated beyond his capacity to think, like so many other arrogant, pompous numskulls who call themselves "scientists." What a psychocircus!

Robert Arnold
West Covina, California

✍ ✍ ✍

**Special Interest Politics**

I am encouraged by your notation about a private briefing with Libertarian Presidential candidate Harry Browne (*IE*, No. 29, p. 68). The best possibilities for unrestricted science and innovation are in free markets.

Hot fusion is not the only failed program sustained by special interest politics. The powerful meat, dairy, sugar, dry cereal industries, etc. are sustained by the Department of Agriculture's Diet Pyramid, which according to extensive research data (that the DOA knows about) supports harmful levels of fat, protein, etc. Superior health requires a low-fat, plant-based diet—which I can attest to after fourteen month's experience.

I enjoy *Infinite Energy* magazine. Although not a scientist, I appreciate the necessity of clean, abundant energy as the lifeblood of progress. I am apprehensive about *Infinite Energy's* inclination toward centralized systems, but at least *Infinite Energy* leaves the door open toward de-centralized systems—which to me is far more realistic, considering power outages and dangerous ugly power lines blighting our communities, not to mention the special interest politics behind centralized systems.

John R. Boyd
Reno, Nevada

Editor's Note: Thank you for your thoughts, but how did you get the idea that we have an "inclination toward centralized systems"? We are all for de-centralizing power generation to home-based units — the forté of cold fusion and new energy. —EFM

✍ ✍ ✍

**Thanks for New Renewable Section**

Thank you for printing "An Opening to Renewable Energy Friends" in *IE* No. 30. I have been a supplier of wind and solar-electric power systems since 1977, when "normal" people thought it was ridiculous. We have much in common and much to share. I would even say we are in the same field, and that you represent the exploratory front.

I want to emphasize the value of small power systems as a way to ease into the market. Consider the history of solar photovoltaic (PV) power. PV panels became an industrial commodity in 1980. A panel that put out 30 watts in full sun cost $500 back then. However, it was easy to install, long-lasting, and trouble-free. By using deep-cycle batteries to provide storage and motor-starting surges, and by using the most efficient lights and appliances that we could find or devise, we were able to sell systems way back then to people who had no electric service to their home, even some low-income people. Now, millions of small solar-electric systems are in use worldwide by private, commercial, and government entities.

Our industry supplies technology to allow small power sources to light homes, refrigerate, communicate, pump water wells, and more, using AC or DC, at a small fraction of the power consumed in the average American home. We will be happy to embrace your developments when the time comes, and to prove that a few watts can go a long way. This is why I subscribe to your magazine. Keep it up.

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**Hydrogen Wells**

In "Sulfur-Deprived Bacteria Produce Hydrogen" (*IE*, No. 31, Briefs, pp. 37-38) it is stated that "...on Earth—no hydrogen 'mines' or 'wells' exist here."

The reference is incorrect. I devote three pages in my book, *Expanding Geospheres* (1992, Polar Publishing), to the mid-continent hydrogen wells, quoting from a longer article by R.M. Coveney Jr. *et al.*, entitled "Serpentinization and the Origin of Hydrogen Gas in Kansas" (1987, *AAPG Bull.*, **71**, 1, pp. 39-48). I list five wells, one in California, two in Kansas, one in Michigan, and one in Iowa with hydrogen contents from 26.0 to 96.3 mol-%.

Where Coveney attributes the hydrogen to serpentine dissociation, I attribute it to effusion of hydrogen from the interior of the Earth through crustal rifts. The subject of hydrogen effusion is the subject of my aforesaid book and also of a Russian author, V.N. Larin, in his book, *Hydridic Earth* (1994, Polar Publishing), which I edited. Hydrogen gas and metal hydrides, especially the hydride of silicon, silane ($SiH_4$) result in the widespread occurrence of hydrogen in wells.

Hydrogen rises copiously through the Earth's great rifts, but has generally gone unrecognized, its presence not suspected. I have made the case that, where hydrogen effuses accompanied by common gases, which may include methane, silane, carbon dioxide, nitrogen, and noble gases, the silane will react with any water it encounters in the crust. The reaction oxidizes the silicon to $SiO_2$ and releases hydrogen and heat. This heat causes volcanism and magmatism, according to my theory, and renders regions of volcanism non-prospective for hydrogen retention or production.

On the other hand, where silane is absent from effusing gas, there is no heating of the environs, and entrapped gas should be producible. Obstacles to successful hydrogen prospecting start with the initial difficulty of getting an unorthodox idea financed. After that, exploration would face the problem of fugacity (a term popular a century ago in describing oil and gas in legal documents), that is the ability of hydrogen to diffuse through caprocks that retain hydrocarbon gases. So, what would the traps be like? What rock barrier would hold back enough hydrogen to constitute a reservoir resembling known methane reservoirs? Today, we don't know. Neither will we find the answer until the exploration effort is made.

Exploration is appropriate in the mid-continent rift area where hydrogen wells are already known. Rift depressions that are not associated with volcanism such as Lake Baikal and the Dead Sea come to mind as prospective. Linear trenches such as the Rocky Mountain Trench and Tintina Trench represent lithosphere faults through which gas should effuse. Areas of identifiable hydride effusions and low heat gradients could be the best potentials. This would point to the Athabasca tar sands region of Alberta, Canada, the largest oil seep on Earth.

I have a 1650 meter (5400 foot) well into the granite shield under the Athabasca tar sands, in which hydrogen was logged in the mudstream all the way down and

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porous zones occur at depths that have not been tested. Attempts to attract financing to test for hydrogen and hydrocarbons have been snubbed by the usual deafness of orthodoxy, despite the fact that hydrogen generation for tar sands production proceeds at a high rate nearby!

Anyone interested in a hydrogen source may want to contact the undersigned (see page 70 for author information). This well is available.

C. Warren Hunt
Calgary, Alberta, Canada

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**Praise for Issue 31**

I just received my *Infinite Energy*, Volume 6, Issue 31. The last few issues of *Infinite Energy* are simply outstanding in all respects. Great articles. Great graphics. Keep up the good work!

Bill Cantrell
Haslet, Texas

✍ ✍ ✍

**Aether**

The editorial by Jeffrey D. Kooistra on aether, which appeared in Issue 31, is interesting but essentially covers the classical picture and a recent paper of A.J. Niemi. There have been important developments in the last thirty years, particularly in the formulation of "Quantum Superfluid Aether." I have been associated with some of these researches. I am listing below a few papers:

**1.** Sinha, K.P., Sivaram, C., and Sudarshan, E.C.G. 1976. "Aether as a Superfluid State of Particle-antiparticle Pairs," *Foundation of Physics*, **6**, 65.

**2.** Sinha, K.P., Sivaram, C., and Sudarshan, E.C.G. 1976. "The Superfluid Vacuum State, Time Varying Cosmological Constant, and Non-singular Cosmological Models," *Foundation of Physics*, **6**, 717.

**3.** Sinha, K.P. and Sudarshan, E.C.G. 1978. "The Superfluid as a Source of All Interactions," *Foundation of Physics*, **8**, 823.

**4.** Sinha, K.P. and Pramana, Jr. 1985. "Bose Condensation of Particle-antiparticle System," *Journal of Physics*, 25, 467.

**5.** Sinha, K.P., Sudarshan, E.C.G., and Vigier, J.P. 1986. "Superfluid Vacuum Carrying Real Einstein-de Broglie Waves," *Phys. Lett.*, 114A, 298.

The reincarnation of the aether has taken various forms, for example, the background vacuum state of all fields. However, the superfluid vacuum state pervading the entire universe is not only the source of all fields and the concomitant interactions, but its superfluid nature makes it consistent with the negative Michelson-Morley result. Also there is no aether drag effect. It can give rise to the large scale structure of the universe. This is facilitated by the fact that the superfluid (superconducting) state will have defects such as vortices and flux lines. In the context of the universe, these form cosmic strings (superstrings) which aid the formation of galaxies and other structures.

There are many other ramifications which cannot be discussed in this letter. However, let me point out a few consequences. The quantum nature of the superfluid vacuum gives rise to the zero-point energy, the quantum entanglement (and non-locality seen in quantum measurement) find ready explanation, and the uniform distribution of matter (energy) in the background accounts for the missing or dark matter. By adding another degree of freedom, such as choice, to each particle and antiparticle (soliton-antisoliton) even the phenomenon of mind and consciousness comes within the embrace of Science.

Dr. K.P. Sinha
Rockville, Maryland

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**In Search of the Mass of the Photon**
**A Letter Sent to *JNE***

The Winter 1999 issue of *The Journal of New Energy* is a special issue entirely devoted to the consolidation of the O(3) electrodynamics as developed by the Alpha Foundation's Institute for Advanced Study (AIAS).[1] One of the predictions of the AIAS O(3) electrodynamics is that the photon has mass.[2] First, AIAS states that the mass of the photon is less than $10^{-68}$ kg = $10^{-65}$ g.[3] Then, AIAS states that the mass of the photon is larger than the masses of the *W* bosons = 80 GeV = 86 g.[4] So we encounter the first contradiction of the AIAS O(3) electrodynamics. This contradiction cannot be resolved because the values between $10^{-65}$ g and 86 g have been excluded.

In an attempt to bolster the prediction of a photon with mass, AIAS presents the equation[5]

$$E_x = Km_f = \frac{1}{\alpha} Zc^2 \hbar m_f \tag{1}$$

Dimensional analysis of this equation produces

$$(\mathrm{eV})_{E_x} = (\mathrm{dimensionless})_{\alpha^{-1}Z} \left(\frac{\mathrm{meter}^2}{\mathrm{second}^2}\right)_{c^2} (\mathrm{eV}\cdot\mathrm{second})_{\hbar} \left(\frac{\mathrm{eV}}{\frac{\mathrm{meter}^2}{\mathrm{second}^2}}\right)_{m_f} \tag{2}$$

$$\mathrm{eV} \neq \mathrm{eV}^2 \cdot \mathrm{second} \tag{3}$$

Since the dimensions don't equate, then this equation is obviously false.

AIAS also presents the equation[6]

$$E_x = \hbar\nu = \frac{1}{\alpha\epsilon_0}\frac{e^4}{8h^2}\left(\frac{m_{el}}{l^2} - \frac{m_{en}}{n^2}\right) \tag{4}$$

Dimensional analysis of this equation produces

$$\mathrm{eV} = \frac{1}{\frac{\mathrm{coulomb}^2}{\mathrm{newton}\cdot\mathrm{meter}^2}} \frac{\mathrm{coulomb}^4}{\mathrm{newton}^2\cdot\mathrm{meter}^2\cdot\mathrm{second}^2}\left(\frac{\mathrm{joule}}{\frac{\mathrm{meter}^2}{\mathrm{second}^2}} - \frac{\mathrm{joule}}{\frac{\mathrm{meter}^2}{\mathrm{second}^2}}\right) \tag{5}$$

$$\mathrm{eV} = \frac{\mathrm{coulomb}^2}{\mathrm{newton}\cdot\mathrm{second}^2}\left(\frac{\mathrm{newton}\cdot\mathrm{meter}\cdot\mathrm{second}^2}{\mathrm{meter}^2}\right) \tag{6}$$

$$\mathrm{eV} \neq \frac{\mathrm{coulomb}^2}{\mathrm{meter}}$$

Since the dimensions don't equate, then this equation is also obviously false. Further, since both equations are false, then the theoretical values presented in the table[5] are also false. Finally, since the theoretical values in the table are false, then any comparison with experimental observations is false and any claim that the hypothesis matches experimental observation to within 1% is false. Thus, there is no experimental evidence that photons have mass.

—Gerald Lindley, Manchester, CT


**1.** Alpha Foundation Institute for Advanced Study, 1999. *The Journal of New Energy*, **4**, 3, Winter.
**2.** *Ibid.*, 167, 168, 178, 180, 181, 216, 221, 234, and 287.
**3.** *Ibid.*, 221.
**4.** *Ibid.*, 287.
**5.** *Ibid.*, 276.
**6.** *Ibid.*, 276.
**7.** *Ibid.*, 279.


**Journal of New Energy Response**

Thank you for calling our attention to a problem in the recent issue of the *Journal of New Energy*. We welcome professional comments. However, you will need to revise your letter a bit:

In your first paragraph you have computed 86 g. The correct dimensions for this calculation is 86 times the mass of the proton (86 proton masses). As Dr. Jin points out: the 80 GeV corresponds to a mass of 80 GeV x 1.6 x $10^{-19}/c^2$ = 1.42 x $10^{-25}$ Kg, which is approximately equal to 85 times the mass of the proton.

For a more reasonable statement on the mass of the photon, please see *Extended Electromagnetic Theory*, by B. Lehnert and S. Roy (World Scientific, 1998). The authors state, "The upper bound on the photon mass was estimated as approximately $10^{-65}$ grams. The recent compilation of laboratory data established that the photon mass should not exceed $10^{-24}$ eV or even $10^{-26}$ eV."

You are right. The equations (3) and (4) on page 276-277 are dimensionally not correct. We will let the author(s) check on that and submit an addendum.

Hal Fox, Editor, *Journal of New Energy*

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# In Memoriam: Giuliano Preparata

The death of Italian physicist Giuliano Preparata at Frascati (near Rome) was announced on April 24. During the last few years he had been struggling with the increasingly severe consequences of the spread of cancer while maintaining a full program of work which, latterly, ranged from chemistry to astrophysics.

*Photo courtesy of Emilio Del Giudice*

During the 1960s and 1970s, Giuliano Preparata made extensive contributions to particle and nuclear physics in what has become known as the "Standard Model of Elementary Interactions." According to this model, matter is composed of two parallel classes of particles, the leptons (including electrons and neutrinos) and quarks (which aggregate to form the hadrons, such as protons, neutrons, and mesons). Whereas leptons are "free," quarks are rigorously "confined." Giuliano Preparata found an explanation of this dichotomy in the behavior of the fluctuations (the "quantum fluctuations") which traverse the "quantum vacuum." (Classical physics describes the vacuum as a deep dark, whereas quantum physics regards the vacuum as a deep dark traversed by sudden, short-lived "lights," the quantum fluctuations.) The incoherent fluctuating noise becomes mutually tuned under suitable conditions, thereby establishing a giant coherent field which erects the barrier required to prevent the spreading of the quarks.

In recent years, Giuliano Preparata and his colleague, Emilio Del Giudice, have extended these ideas to the atoms and molecules of "ordinary matter." In this case the coherent electromagnetic field arising from the quantum fluctuations is the prime cause of the developing structure and energetics of liquids and solids (in distinction to the properties of gases). In due course, Giuliano Preparata initiated a research program devoted to the consequences of these ideas within the Italian State Agency for Energy (ENEA), the main initial effort being devoted to the effects of the so-called "cold fusion."

It is undoubtedly true that the concepts advanced by Giuliano Preparata remained somewhat unfashionable during his lifetime. It is also true that he was widely regarded as having a difficult temperament, an opinion sparked by his scrupulous honesty. At the same time, there is a wide group of people who appreciated his integrity and generosity of spirit. He became increasingly appreciated as a key figure who could lead the next phase of research in the natural sciences at the start of this next millennium. His contributions to the field will be sorely missed.

Giuliano Preparata is survived by his wife, Emilia, and his son, Guido. We mourn his passing.

*Martin Fleischmann*


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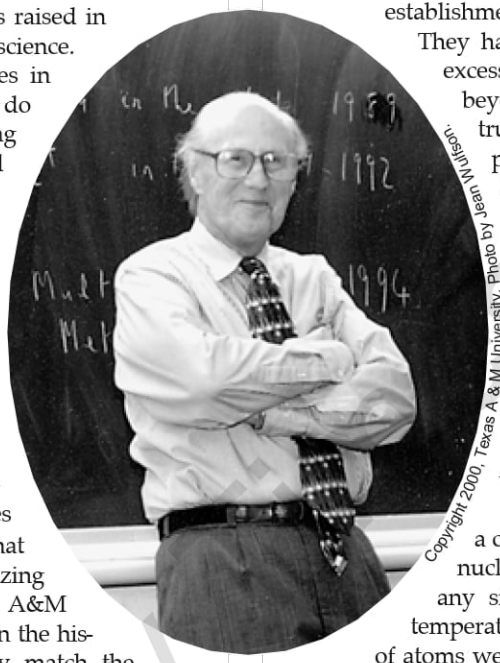
# The Triumph of Alchemy: Professor John Bockris and the Transmutation Crisis at Texas A&M

**by Eugene F. Mallove**

Distinguished Professor of Chemistry at Texas A&M University, John O'M. Bockris, is one of the top two or three electrochemists of the twentieth century. He must be counted as a lineal intellectual descendant of one of the greatest scientists of all time, Michael Faraday, who was of humble birth but became a towering figure of nineteenth century science. Like Bockris, Faraday was raised in England and came to love many facets of science. In addition to his fundamental discoveries in electromagnetism, Faraday had much to do with the birth of electrochemistry. Among other things, he named the process called electrolysis, designated the anode and cathode, and coined the term electrolyte.

In a multifaceted scientific career from the 1940s through the present on three continents, Bockris and his student protégés pioneered many of the current directions in electrochemistry, and they confirmed several aspects of the Fleischmann-Pons cold fusion experiment.

Bockris was born January 5, 1923 in Johannesburg, South Africa—an extensive review of his lifetime of scientific work leaves no doubt about his prominence.[1] The story that follows is but a segment of that life, the amazing saga of John Bockris' experiences at Texas A&M University during a major paradigm shift in the history of science—one that may eventually match the impact of the Copernican revolution. Though that assertion may anger or amuse critics, history will be the final arbiter.



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### The Beginning: Cold Fusion

Although often overlooked in the glare and glitz of other branches of chemistry, such as biochemistry or fullerene (Buckyball) chemistry, electrochemistry is important to our industrial civilization. It turned out to be far more significant than anyone could have imagined prior to March 23, 1989. On that day, a scientific shock was felt around the world. It may lead to the end of civilization as we know it—the end of the age of fossil fuels and much else we could do without.

One of the students whom Bockris influenced at Imperial College in London when he was a Professor there (1945-1953) was Martin Fleischmann. Decades later, Fleischmann and Stanley Pons (who had been a student of Fleischmann's) announced at the University of Utah one of the most astonishing and bitterly contested discoveries in the history of science. Their finding, later verified by numerous laboratories around the world, became known as "cold fusion"—nuclear-scale excess energy in electrochemical cells that incorporated heavy water with palladium and platinum electrodes. Furthermore, because of the large energy release and absence of chemical "ash" to explain the reaction, the cold fusion discovery was immediately seen as a means to extract unlimited amounts of energy from abundant water. The scientific, technological, economic, geopolitical, and social implications were immense. It was as fundamental a discovery as the domestication of fire.

As a presumptive nuclear process, with originally poorly understood nuclear reaction pathways, cold fusion came into direct competition with the well-funded "hot fusion" establishment. Plasma physicists and engineers had been trying for decades to mimic the cores of stars, using controlled thermonuclear reactors at universities and government research establishments. They had had only marginal success. They had never achieved even a single watt of excess power out of their mammoth machines beyond the electrical power that was put in. In truth, they had little hope of achieving any practical working device before another half-century, if then, after billions of dollars more were spent. That they would not be the heroes who would rescue civilization from an energy crisis looming in the next century was too much for the hot fusioneers to accept. By contrast, though initially low power, some cold fusion experiments indicated high percentage excess power even in some very primitive initial forms, without the lethal neutron radiation of hot fusion.

Cold fusion was also an affront to almost a century of prevailing scientific wisdom that nuclear reactions could not possibly occur to any significant extent near ambient terrestrial temperatures. The highly positively charged nuclei of atoms were, it was said, unreachable by other positively charged nuclei, such as those of hydrogen and its isotopes. Above all, cold fusion was an assault on the current smug, self-assured high energy physicists—the veritable high priests of science who claimed to know almost everything about the fundamental laws of physics except a few remaining puzzles. They aimed for and wrote about a "Theory of Everything" that physics would finalize in their lifetimes. Just give them a few tens of billions of dollars more to build the giant Superconducting Supercollider (SSC)—ironically, under the plains of Texas, in the state where Bockris worked—and they would create this eternal edifice. That they could be utterly wrong about what some bench-top chemists, such as Bockris, Fleischmann, and Pons could have achieved, was unthinkable. The "Church of Science" had spoken. Cold fusion was branded "pathological science," "bad science," and "fraud."

In 1989, John Bockris and his students at Texas A&M University immediately fell into the whirlwind of activity surrounding the Fleischmann and Pons announcement. Hundreds, perhaps thousands, of scientists all over the globe struggled in the spring of 1989 to master the process—or prove it wrong. Bockris was to be rewarded with some of the earliest and most fundamental initial discoveries in cold fusion—in particular, that the radioactive form of hydrogen—tritium—could be produced in unexpected bursts within cold fusion cells. He and his colleagues later determined that helium-4 was produced in nuclear reactions in layers below the surface of palladium cathodes; others found helium-4 at the cathode surface. For his tritium discovery, in 1990 Bockris and his students were rewarded with a brutal press assault.

Science journalist Gary Taubes dragged Bockris through the

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*Bockris was to be rewarded with some of the earliest and most fundamental initial discoveries in cold fusion—in particular, that the radioactive form of hydrogen—tritium—could be produced in unexpected bursts within cold fusion cells.*

mud, accusing him of being naive about fraud in the alleged spiking of cold fusion cells with tritium by one of his students. Taubes' attack was without foundation, but it was given wide currency by the main organs of the scientific media. It was repeated by other opponents of cold fusion, who were not bold enough to make the charge of fraud directly. Some of Bockris' colleagues at Texas A&M University participated in the witch hunt.

A few years later, the whole sordid affair against Bockris took a remarkable turn. The openness to new ideas about low-energy nuclear reactions (LENR) that the cold fusion discoveries had given to its investigators led Bockris onto even more heretical ground. Through a series of events with a cast of characters and circumstances that a Hollywood scriptwriter would be hard-pressed to conceive, Bockris began to test the claims of ancient alchemy—the transmutation of heavy metals, such as mercury, into gold and other elements.

Two amazing things happened, one predictable, one not. It was 100% predictable that when the larger world found out about the transmutation work at Texas A&M, along with the antics of the strange people who introduced Bockris to it, there would be an even more violent witch hunt against Bockris and his work. What had not been foreseen, however, was that some of those transmutation experiments apparently worked and became part of a growing body of experimental evidence that heavy-element transmutation at significant levels was possible in cold fusion and in cold fusion-related experiments. (For some time it was difficult to persuade even some "mainstream" cold fusion researchers that the evidence for heavy-element transmutation was in hand.) It soon became clear that radioactivity could be produced in what looked like mere chemical experiments. Radioactivity could be reduced or destroyed in similar experiments, and new stable elements and isotopes across a vast spectrum of atomic mass could be produced in heretofore exclusively chemical experiments. Had scientific alchemy risen from the grave—from its earlier death at the hands of twentieth century establishment science? Yes. John Bockris was one of the leading attendants at its rebirth.

What follows is the story of the scientific and personal courage of a great scientist under a most withering and unfair attack against the free experimental investigation of nature. The persecution of John Bockris at Texas A&M University hearkens back to the treatment of Galileo at the hands of the Catholic Church in the early seventeenth century. But this was near the end of the twentieth century, when scientists had supposedly assimilated those lessons of suppression and ridicule by inquisitors of old.

In some ways, the modern inquisitors were worse than their predecessors of the past: they had no excuse for their actions. They were not ignorant. They had seen and read about other historic paradigm shifts within science, but it seems they had forgotten the lessons, or they never really believed them. These modern-day anti-scientists with sheepskin certifications from academe were claiming that they were "objective." They said it was the purveyors of "pathological science," such as Bockris, Fleischmann, and Pons, who were polluting and betraying objectivity within science. By fiat of majority vote, they could say who was deemed a good scientist, who bad, what was possible to expect from nature's microcosm, and what was not. In short, this was tyranny within the house of science.

**TAMU**

Texas A&M (affectionately, "TAMU"), a large university of about 45,000 students, is located in the small town of College Station, which adjoins the old town of Bryan. It is well-known for its football team, "the Aggies," and its ROTC (Reserve Officer Training Corps) program.

The University has a great endowment, one of the largest in the country, from land donated to it in the nineteenth century, before it was known to contain oil. And, there are great plans for Texas A&M to become one of the ten leading universities in the country. Some departments indeed have world leaders in their fields. Though the official major emphasis is on agriculture and engineering, there is a Chemistry Department which ranks among the leading ten in the country, as measured by the activity of the graduate schools.

One of the features of Texas A&M, certainly exemplified by the Chemistry Department, is to appoint famous professors who are well on in their careers and who will lend instant distinction to their department. In 1997 there were seven Distinguished Professors in Chemistry out of a university-wide total of about twenty-eight active (*i.e.* non-retired) Distinguished Professors.

Bockris joined the Department of Chemistry at Texas A&M in 1978, a time when he had already authored 406 published technical papers and several noted books. He had begun his academic career at Imperial College of Science and Technology in London (1945-1953), then he was on the faculty of the University of Pennsylvania (1953-1972), and at Flinders University of South Australia (1972-1978). Between 1978 and 1992, when calls for his demotion or ouster began at Texas A&M, he had published some 250 papers at that university. For physical chemists, this is a very good record. Bockris had been very active in getting research grants. From 1979 until 1991, he ranked first or second in total research funds contributed each year to the Chemistry Department. Much of this money came from private sources, since the National Science Foundation (NSF) has no program in physical electrochemistry.

**Tritium in the Cold**

The Fleischmann and Pons announcement of cold fusion (actually the *re-discovery* of a primitive 1920s finding) happened on the afternoon of March 23, 1989. Bockris learned about it the next day, having missed the evening television reports. The young Martin Fleischmann had been a graduate student at the Imperial College of Science and Technology in London when Bockris started there as a lecturer in 1945. Fleischmann and his family, by the way, had escaped Czechoslovakia just before World War II. Since the field of top electrochemists in the world is fairly closely knit, it was easy for Bockris to call Fleischmann and ask him what was going on. Fleischmann told Bockris a few things about the way he and Pons prepared their electrolyte and the techniques they used to attain the unusual excess heat. Fleischmann considered the excess heat to be of nuclear origin, because of its high magnitude and lack of chemical explanation. But there were also associated—albeit initially weak—signs of nuclear activity, such as tritium and neutron production. This telephone input immediately triggered the cold fusion research in the Bockris group.

At the time Bockris was supported by a number of sources, especially the Electric Power Research Institute (EPRI). For a month or two, Bockris turned his whole group to trying to confirm or reject the Fleischmann and Pons claim. The temporary

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deviation from existing programs was encouraged by EPRI. The group sought to observe excess heat and tritium, and in these few weeks it manned a round-the-clock effort without formal contracts, which would have taken months to engage.

Because Texas A&M had a thermodynamics group, several electrochemical groups, and a strong nuclear science organization, it was an ideal university for EPRI's purpose. Bockris had encouraged EPRI to fund several groups at Texas A&M; indeed, EPRI funded three groups in Chemistry, one in Chemical Engineering, and one in the Center for Electrochemical Systems.

The first act of the unfolding drama was connected with a graduate student in Bockris' department, Nigel Packham. He and several others had been taking samples of the solutions of heavy water and lithium salts, which had been electrolyzed on palladium, to the Nuclear Engineering department. There tests were made on samples of electrolyte for the presence of tritium, the radioactive isotope of hydrogen, which has a decay half-life of 12.3 years. Bockris' group thought that it was important to look for this, because if the solution consisted of deuterium oxide (following the Fleischmann and Pons methodology) one of the most obvious pieces of evidence for nuclear activity would be tritium formation. Bockris realized that helium might also be produced, but its detection was beyond the capabilities of the group at the time.

One of the groups that was funded in parallel to Bockris' was led by Charles Martin, a professor in the electroanalytical chemistry division. His students were enthusiastic too and went to the same place to test their samples for tritium. Packham and others took numerous samples to Nuclear Engineering without any tritium being detected, but sometime in May 1989, Packham reported that the operating technician said, "What have you done with this one?" It contained a large concentration of tritium, in the 1,000 dps (disintegrations per second) range. The group had taken four samples from the solution at different times, and these results showed tritium climbing to an asymptote, *i.e.* the tritium production stopped after a few hours. The staff of Bockris' group agreed that someone had been present in the laboratory all the while that the tritium had been emerging. The research activity occurred during the day. However, it took about 400 hours of electrolysis for the electrode to begin to produce tritium. The Bockris team quickly put together a note for publication in the *Journal of Electroanalytical Chemistry*. It was returned twice for revisions but was finally accepted for publication. It was the first published account of tritium formation in a refereed journal and one of the first confirmations of the claims for a nuclear reaction "in the cold," made by Fleischmann and Pons.

Bockris' team announced that they had produced tritium at the meeting on cold fusion, held in Santa Fe, New Mexico under U.S. Department of Energy (DOE) auspices in May 1989, which encouraged everyone. The group continued to observe tritium sporadically for the next two years. The Bockris group devoted three reports to tritium formation. One of these contained a comparison of tritium and excess heat. It showed that the amount of tritium being produced was far too small to account for the heat by hypothesized nuclear reactions. In 1991, it was shown by Miles *et al.* of the U.S. Navy that helium was also being produced, but at a rate nearer to that needed to account for the production of the excess heat. This year, McKubre at SRI International and two groups in Japan *confirmed* helium production commensurate with excess heat production. (See Report on ICCF8, p. 25.)

The number of experiments at the Bockris lab devoted to investigating tritium production was 58 and the total number of times tritium was observed was 18, with 40 failures. In retrospect, Bockris wondered, had they left the other 40 cells to run more than 500 hours each, might they all have produced tritium eventually?

**The Scandal Monger**

Enter science journalist Gary Taubes, who had received a contract early on with Random House to write a book about cold fusion. He already privately believed cold fusion claims were "pathological science" and perhaps even fraud. Taubes visited the Bockris group where Nigel Packham was doing the research. Bockris initially thought that the tall, imposing Taubes was a genuine seeker of the truth, so he let him see everything the group had, including notebooks. Bockris discussed with Taubes the various pluses and minuses of the work in a spirit of openness. In the beginning, Taubes behaved normally, jotting notes and tape-recording the conversations with Bockris and others on his staff.

Bockris would later learn that Taubes had visited Texas A&M a second time without seeing him. Moreover, Taubes had also gone to London, England to investigate what Bockris had told him about the family background of Nigel Packham. He had interviewed Packham's parents and developed what turned out to be an utterly misguided theory of sinister motivations that might have impelled Packham to engage in scientific fraud—spiking the experimental cells with tritium.

A curious connection: Confirmation of this delusion of Taubes came to me in March 1990 at the First International Conference on Cold Fusion (ICCF1) in Salt Lake City. Taubes excitedly told me that he had developed an extensive psychological profile of Packham, which pointed to him being a fraud perpetrator within the Bockris lab. The alleged profile seemed to me then to have no bearing on the integrity of Packham, whom I would later meet and come to respect. Taubes' purpose was to get me to ask Bockris during a technical session at the meeting an embarrassing question about possible fraud in his lab.

Taubes thought that he could establish that Packham had never been a graduate student at Imperial College. When he learned about this preposterous notion, Bockris immediately obtained by fax from Imperial College Packham's registration papers for the graduate program in the School of Electrical Engineering.

Taubes made a third visit to Texas A&M in which he adopted a different public attitude. Now he was extremely aggressive, telling Bockris that the tritium results had been falsified by Packham. He said that other workers, particularly those in Charles Martin's group, had not been able to observe tritium. He suggested that Packham had falsified his results because he wanted to impress Bockris and get his Ph.D. more quickly.

Bockris remained calm under this attack. Taubes suggested to Bockris that he had played some part in the alleged fraud, because he had hoped for increased funding, which surely would be the result of the acceptance of such a remarkable

*In some ways, the modern inquisitors were worse than their predecessors of the past: they had no excuse for their actions. They were not ignorant. They had seen and read about other historic paradigm shifts within science, but it seems they had forgotten the lessons, or they never really believed them. These modern-day anti-scientists with sheepskin certifications from academe were claiming that they were "objective."*

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claim. Bockris showed Taubes the record of all his research grants (eleven at the time), which proved that he had plenty of research funding. Bockris somewhat naively suggested to Taubes that he should talk directly with Nigel Packham, and he would doubtless be able to see the actual documented progress of the research in the lab notebooks. Taubes did just that, but afterwards, Nigel Packham strode into Bockris' office and exclaimed, "This man wants blood!"

Taubes had threatened Packham after he had talked to him for some time. He had told him that he should "confess" to having put the tritium in the solution from a supply of tritiated water that was in the lab. Packham told Bockris of the Taubes threat: If he (Packham) were to confess right then in a tape-recorded interview, Taubes would not publish anything about it until he wrote a book demolishing the "myth of cold fusion."[2] Packham would then have six to nine months to find a job. On the other hand, if Packham was *not* willing to "confess" right then and there, Taubes would quickly publish an article in *The New York Times*, where he had connections, that fraud was being committed in the Bockris laboratory by Nigel Packham. Packham's career would be ruined and with it some of the most important emerging scientific evidence for cold fusion.

The Taubes threat was extremely serious, especially since the issue of scientific misconduct was becoming a rather fashionable topic at the time in academia and in science journalism. It seemed that Taubes would stop at nothing to boost the prospects for his eventual book. Yet despite Taubes' attitude, Bockris invited him to lunch at his Club. There Taubes talked about his life, his exploits writing exposés for *Discover* magazine of less-than-angelic scientists, and his activity in writing scripts for Hollywood movies. Taubes had written a book, *Nobel Dreams*, in which he had attacked the reputation of famous Nobel laureate Carlo Rubia, a leader at CERN, the high-profile European center for nuclear research. (Rubia, it was rumored among science journalists, came close to suing Taubes.) Taubes swiftly departed the meeting with Bockris at TAMU, allegedly to get his article into *The New York Times* or elsewhere.

Despite the emerging threat of a scandal, Bockris recalls that his attitude at the time was rather casual. He knew what the group had done and there seemed to be little that Taubes could do to substantiate a non-existent fraud. But it was clear that Taubes was determined to prove his case in the press with circumstantial evidence; the false accusations would be damaging.

No article appeared the next day in the *Times*, but a short time later Bockris received a call from London from the powerful editor of *Nature* magazine, John Maddox. In his cultured English accent, he told Bockris quietly that a paper had been received by *Nature*, which claimed that fraud was being perpetrated in the Bockris lab. Maddox wanted a comment from Bockris himself. In the same reserved British tones, Bockris replied to Maddox that there was certainly no fraud. The work referred to was being carried out by an English graduate student from Imperial College, Nigel Packham, and other students and postdocs. Bockris told Maddox that although his group had been the first to publish a paper on tritium production in 1989, there were now several other independent groups that had found the same general result, notably Srinivasan's team at the Bhabha Atomic Research Centre in India (BARC). Bockris asked Maddox to forward the article for comment. Maddox agreed to fax it the next morning.

It was not pleasant that evening for Bockris. He went home to his charming wife Lilli (an Austrian-born Jewish woman who had escaped the Holocaust), knowing that he and his students had been accused of fraud at the most famous scientific publication in the world. It was not a restful night. When he returned to his office the next morning, he expected to find the article sprawling out of the fax machine, but nothing had come through. He waited until 4:00 p.m. London time and called Maddox to learn what was happening. Maddox's secretary said that he was in conference with lawyers and could not be disturbed.

Bockris called back an hour later and was told, "Dr. Maddox will call you soon." Finally, Bockris did get a call from Maddox, whose attitude had changed markedly. Bockris recalls the situation was like that of a "pricked balloon." Maddox spoke to Bockris in a tone of resignation: "We have put the article on the back burner." For what reason? Apparently *Nature's* lawyers had raised objections to its publication, as, indeed, they should have. (The accusations of fraud finally did come out in *Nature's* competitor magazine, the U.S.-based *Science*.) *Nature's* decision to reject the Taubes story had come as an immense relief to Bockris.

At the end of this second telephone discussion between Maddox and Bockris, Maddox asked hesitantly, "You say there are others who find tritium?" Bockris replied affirmatively, mentioned four groups that had detected tritium in cold fusion experiments by then, and sent Maddox the references and a report. Unfortunately, Maddox failed to investigate the matter of tritium production or any other discoveries within the emerging low-energy nuclear reactions field. *Nature* continued its attacks on cold fusion science and scientists, and to this day holds an anti-scientific position on cold fusion. Ironically, Maddox would later write a popular book titled, *What Remains to be Discovered* (1998). No mention of cold fusion in that book, of course!

**Shock Treatment**

Then came a great shock. At that time, the groups working on cold fusion at Texas A&M (one in Chemical Engineering, associated with the Thermodynamics Research Center; two groups in the Chemistry Department; a group in the Center for Electrochemical Studies in Hydrogen; and a group in the Nuclear Science Division) met once every two weeks to compare results. At one of these conferences, Professor Kevin Wolf made a startling announcement that would throw the whole matter of tritium detection at Texas A&M into disarray.

First, some essential background about Wolf and his association with the Bockris group. The late Kevin Wolf (who died unexpectedly in 1997, at age 55, see Obituary, *IE*, No. 18, p. 42) had been a well-known, well-respected nuclear chemist. Today, Wolf in many ways is still highly regarded within the cold fusion field, despite what many interpret as his unfortunate actions in 1990 and beyond. Wolf had received plenty of research support from the Department of Energy and other sources. He had been chosen by EPRI to be the recipient of the greatest amount of money that that prestigious organization was directing into Texas A&M for cold fusion research.

Kevin Wolf worked with Bockris' group, which had no nuclear chemist on staff. (Tom Schneider at EPRI had given the Bockris group $27,000 to buy a scintillation counter to measure tritium, in addition to the EPRI money already granted.) When the Bockris team began to observe tritium, the group needed someone who knew nuclear chemistry and tritium measurement. In the early days of cold fusion people suspected that the tritium was "coming from somewhere else." Tritium, created "in the cold" was regarded as impossible—yet it was being observed to form in cold fusion cells, without the high-energy (14 MeV) neutrons expected if it were being created even at low-level in an energetic plasma. The positive results for "cold tritium" were so unexpected that scientists wondered whether all sources of contamination by pre-existing tritium had been checked. Tritium was being used in the Chemistry Department in other ways. It was not out of the question that there could be some tritium coming through in the ventilation system.

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Kevin Wolf, knowledgeable in relevant tritium measurement techniques, worked with Nigel Packham and Jeff Wass, another graduate student who was associated with the very early work on cold fusion in the Bockris laboratory. One of Wolf's jobs was to thoroughly examine the lab in which the work was being done. For example, curtains and hangings, floor coverings, and many other items were tested. Wolf seemed to Bockris to be very helpful. Wolf would walk down the Chemistry Department corridor every day (to collect his mail, he said). He often talked with Nigel Packham and Jeff Wass and knew their work intimately.

News & Comment

Cold Fusion Conundrum at Texas A&M

*The administration's laissez faire response to worries about possible fraud raises questions about the proper balance between academic freedom and the need to guarantee the integrity of research*

WHEN TRITIUM FIRST APPEARED in John Bockris's "cold fusion" experiments in late April 1989, the effect was anything but subtle. Overnight the concentration of tritium in the Texas A&M chemist's electrochemical cells increased 10,000-fold. When the tritium appeared repeatedly, in six different cells in one week, it began to look like salvation for cold fusion.

After a year of ambiguous or simply negative experiments, Bockris's tritium data remain not only the single most extraordinary "cold fusion" effect, but also the only compelling evidence in support of the original cold fusion claims. Last June, for instance, it was Bockris's testimony before the Utah legislature, along with that of Robert Huggins of Stanford, that persuaded the state that cold fusion had been confirmed and deserved a $5-million investment. Nine months later, as *Chemical & Engineering News* wrote after the First Annual International Cold Fusion Conference, "[Proponents of cold fusion] point to the observed emissions of tritium as the unmistakable signature of a nuclear reaction."

Yet almost from the beginning, researchers familiar with Bockris's experiment, and not enamored of cold fusion, have suggested that his data were perhaps too good and too easy. How was it that his group, within a month of the original cold fusion announcement, was able to produce tritium in quantities that no other U.S. researcher has come close to, even when following Bockris's recipe exactly? Was it truly a fusion reaction, which would require rewriting nuclear physics? Was it some inadvertent contamination? Or was it something more insidious?

Perhaps inevitably, suspicions were raised almost from the first that the tritium in the A&M cells was put there by human hands. As time went on, even members of Bockris's group would express their doubts about the "miracles" that seemingly favored the team. Other researchers, both at A&M and at outside institutions, warned that questions about possible fraud would have to be resolved before the results could be accepted.

But the response of the A&M researchers and administration to these concerns was limited at best. Instead of taking positive steps to guard their results against fraud, Bockris and his co-workers principally offered arguments as to why they thought fraud was unlikely, sometimes exaggerating their case in the process. And the Texas A&M administration, although it has been aware of some faculty members' suspicions and has kept an eye on the tritium work, has done nothing past some preliminary questioning.

The result is that after a year of experiments that most scientists view with a great deal of skepticism anyway, the A&M researchers are still haunted by this specter of possible fraud. Even Kevin Wolf, an A&M nuclear chemist who worked closely with Bockris on the tritium work, believes that fraud cannot be ruled out as an explanation for the tritium results, although he now believes that inadvertent contamination is to blame for his own results (see box, p. 1381).

Although the origin of Bockris's tritium may not be resolved for years, the tritium episode has become a case study in the damage done when questions of fraud, legitimately raised, are not seriously addressed by either the lab chief or his institution. It raises crucial questions about how rumors and allegations of fraud should be investigated while ensuring academic freedom and protecting the reputations of scientists, whose careers may be at stake. In an atmosphere of increasing public scrutiny of the scientific process by legislators like Representative John Dingell (D-MI), the scientific community must have ready answers for such questions. And they take on added importance in this case, because of its high profile and the tens of millions of dollars and thousands of scientific man-hours spent chasing after the chimera of cheap, plentiful energy from "fusion in a jar."

Bockris's laboratory was one of several hundred worldwide, and three at Texas A&M alone, that began the chase to confirm cold fusion after the public announcement of Stanley Pons and Martin Fleischmann on 23 March 1989.

Pons and Fleischmann reported that they had initiated nuclear fusion in simple electrochemical cells that consisted of a palladium electrode and a platinum electrode submerged in a bath of heavy water. A current, passed through the cells, caused the deuterium ions of the heavy water to be absorbed into the palladium. At that point, Pons and Fleischmann claimed, the density of deuterium was such that two deuterium atoms would fuse together, producing heat and the requisite products of deuterium-deuterium fusion: neutrons, tritium, and helium.

Both their theory and their evidence, however, contradicted much of what was known about deuterium-deuterium fusion. Nonetheless, Pons and Fleischmann were well-respected scientists, and their claims suggested a cheap, virtually inexhaustible source of energy. The stakes were high.

At Texas A&M, Bockris, an old friend of Fleischmann's, began trying to replicate the claims in early April. Bockris's group constructed several dozen cells and began looking for evidence of the excess heat that Pons and Fleischmann claimed could only be explained by a nuclear process at work. But the calorimetric setup, as Nigel Packham of Bockris's group put it, was "primitive as hell." This was where Kevin Wolf entered the picture. A nuclear chemist who was not associated with Bockris, Wolf began checking Bockris's cells for neutron emission with his detectors at the A&M Cyclotron.

Tritium producers. *Fusion cells in the Bockris lab.*

11 JUNE 1990 NEWS & COMMENT

*Science*, **248**, June 15, 1990

All this is by way of background to the stunning announcement which Wolf made in one of the joint cold fusion group meetings. He blurted out that an article was to be published in *Science* in about two weeks (mid-June 1990), which would concern the Bockris group's work on tritium! He said it would be a lengthy article written by Gary Taubes, presenting the conclusions of what Taubes had learned. The main point of the article would be to castigate the administration of Texas A&M for allowing possibly fraudulent and unbelievable results of tritium production.

Wolf's announcement was a shock for two reasons. First, Wolf had to some extent collaborated with a muck-raking journalist, who would bring charges of fraud upon the colleagues whom he visited every day, without alerting them about Taubes' intentions. Perhaps this was the reason for the many unexplained visits by Wolf to the Bockris group

Second, Wolf *himself* had detected tritium in high concentrations in cold fusion experiments. Only months before (March 1990), he had presented a paper at the First International Conference on Cold Fusion (Salt Lake City) in which he claimed to have produced tritium.

At the very time that Wolf announced the forthcoming article in *Science* magazine by Taubes denouncing the Bockris group and the whole College of Science, Wolf sent Bockris copies of letters that he had apparently been secretly writing to Dr. David Worledge at EPRI, the program manager in charge of the Bockris work. He had alleged to Worledge that the Bockris group tritium work must be tainted by fraud. Wolf's tortured reasoning in these letters further revealed his apparent duplicity.

Being familiar with the Bockris laboratory on his daily visits, Wolf had no difficulty in surreptitiously removing a test tube of a solution in which the group had found tritium. Analyzing this in his own lab, Wolf had found some light water in the deuterium oxide (heavy water) solution. This seemed to him to support the idea that Nigel Packham might have put into the solution a significant amount of tritiated water. Wolf also revealed that he had been secretly writing to Dean John Fackler too (see Exhibit B p. 23 - excerpts from an interview with Prof. Fackler), telling him that the Bockris work might be fraudulent, because of the tritiated water which Packham allegedly had added to the solution.

The situation grew more serious by the day. Now the Bockris group was attacked publicly by the most influential magazine of science in the United States, *Science*.[3] It was strongly implied that the group's tritium results had been fabricated.This was being played out in front of the Dean of Science, and would go further up the University administration, because of the high-profile publication in *Science*.

Because Kevin Wolf announced the Taubes *Science* article just days before its publication, Bockris had no chance to do anything about it. There was barely time to write a letter to *Science*, with information that might have prevented the publication, or at least might have significantly modified the piece. Bockris went to see Dean Fackler about the matter. To his amazement, he learned that Fackler had known about the impending article for some weeks, and that Taubes had been talking to him by phone too! The Vice Dean, Abe Clearfield, also knew of the upcoming article. Although Clearfield was a colleague of Bockris in the Department of Chemistry, and Bockris knew John Fackler collegially, neither of them had informed Bockris. When the article finally appeared, it was a long, five-page feature. Length aside, it can be summarized easily: Absurd research was being carried out in the Chemistry Department, which should never have been continued in the first place after the "collapse" of cold fusion, and the work might even be tainted by fraud. The University administration was at fault in allowing this and should have imposed rigorous supervision, if it was going to allow the work to proceed at all.

The *Science* article was careful in that it did not actually say the Bockris group work *was* fraudulent, but much was said to hint in that direction. Prof. Charles Martin was quoted: "I warned Hall [the department head] that I thought there was a very good chance the experimental results were the result of fraud." There was a framed inset "box" in which Kevin Wolf opined that the Bockris work had been sloppy and poorly carried out. (Wolf suggested that his own tritium results might have been from pre-contaminated palladium, a suggestion that was later shown to be without foundation. In other published papers, researchers looked for tritium contamination in many different samples of palladium and found none. One of these papers was authored by Fritz Will, a former president of the Electrochemical Society.) The article was extremely damaging, and its negative implications for Bockris and everyone else working in cold fusion were all too clear. Though the U.S. DOE ERAB Cold Fusion Panel had rendered a rush to judgement against cold fusion in the fall of 1989, continued reports of tritium production, low-level neutron emission, and nuclear-scale excess heat continued to come from laboratories around the world. The Texas A&M work on tritium was one of the most important results, which helped sustain interest in the field during its formative stages. Taubes and his supporters in academe knew that very well, which is why they were anxious to cast doubt on the work.

Bockris' first reaction was to consider legal action. He thought it might be possible to sue *Science* magazine for defamation. He listened to the advice of seven associates and friends. Only one advised that he sue. On the other hand, this

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defamation. He listened to the advice of seven associates and friends. Only one advised that he sue. On the other hand, this dissenter was a law professor at Temple University in Philadelphia, who thought Bockris could easily win a libel suit or receive a settlement. Those who advised against legal action realized that while *Science et al.* could afford a $1 million or more to defend against a lawsuit, Bockris would be strained to pay even one-hundred thousand dollars—perhaps the minimum needed to challenge such powerful media forces.

Bockris chose instead to prepare a reply to the accusations in *Science*, with all the science he could bring to bear—picking apart the statements made by Taubes one-by-one. Bockris called the then editor of *Science* and told him that the article had been false in its implications. The editor was rather cold and said that he was "sorry." Bockris then wrote *Science* to ask if he could reply on so important a matter of science and ethics, with the same space that Taubes had been given. *Science* rejected the suggestion outright, claiming that a detailed reply would not be accepted for publication. Bockris recalls the surprising reason given, roughly this: "The public is interested in fraud, but they are less interested in normal science." Eventually, Bockris was allowed to publish a one-column letter in which he stated the plain facts of the discoveries and denied that there was anything experimentally wrong or unethical.

It was too early for Bockris to put up what would become a major defense, namely numerous replications by others. True, there were already a few confirming papers on tritium when the *Science* article came out, but Bockris had to wait until 1994 for some 147 papers to be published in support of tritium production in the cold. By then, Bockris had stopped counting the papers that claimed successful tritium production.

Several of the people from whom Bockris had sought advice said that his main concern should be his scientific reputation, which would depend on replication of tritium production, suit or no suit. But an accusation of fraud made at a high level does not readily disappear, scientific reports notwithstanding. By the time the huge number of independent confirmations of tritium came in, it was too late. Many had concluded that one or more people in Bockris' group had committed fraud. Cleaning off the mud proved very difficult.

Dr. Edmund Storms of Los Alamos National Laboratory, who could defend his own successful results in tritium production in cold fusion experiments there at the world's foremost tritium measuring laboratory, watched Bockris' ordeal from afar. Independent of Bockris, Storms devised a test which could determine whether or not the Bockris group results could have been due to spiking with tritiated water. If the tritium had been put in by Packham, it would be present in an ionic or molecular form and would remain there independent of time, emitting its tell-tale beta particle radiation. If the results were produced as gaseous DT at the electrode, the absorbed gas would be sparged out by the constantly bubbled $D_2$—the tritium concentration would decline with time.

Storms prepared a graph, based on an experiment, in which he showed the two different behaviors. Storms wrote a letter to *Science* in which he commented that time-history of the Bockris group results constituted clear proof that they were not due to tritium spiking. Tritium activity (radiation counts measured) did decrease with time, if $D_2$ continued to bubble through the solution during electrolysis. The tritium found in the solution had been formed on the electrode as a gas, partly dissolved in the solution and partly rising into the gas phase. *Science* magazine refused to publish Storms' brief article. Faced with proof that no fraud had been committed, *Science* preferred discrete silence and a cover-up rather than admitting that its story was flawed. Perhaps *Science* will someday atone for this ethical travesty of 1990.

Faced with proof that no fraud had been committed, *Science* preferred discrete silence and a cover-up rather than admitting that its story was flawed.

By 1992, Bockris' group had observed tritium many times. Bockris had been working with scientist C.C. Chien from Seoul, South Korea who had himself observed tritium, independently of Texas A&M work, *before* he came to work with Bockris. Working with Chien led on one occasion to a very remarkable electrode, which continued to emit tritium for several weeks. After it had emitted tritium for ten days, Bockris thought that it was reliable enough so that he could call neutral or skeptical colleagues to see the process for themselves. The rate of increase of tritium in the solution was such that one could make two measurements an hour apart and detect a significant increase in the tritium concentration. Bockris planned to tell colleagues in the nuclear science division, "Come and see for yourself. Do a test yourself!" The scintillation counter was in the adjacent room, so Bockris surmised that a colleague could stay with the apparatus for an hour, taking two samples. This would prove that no one was adding tritium artificially during the time of increase.

He phoned the Director of the Nuclear Science Division, who said that he was just about to go to Germany to carry out some research there. He could not come to see the tritium. Bockris phoned another person in the Chemistry Department, who was concerned with trace analysis and part of whose work was nuclear-related. This person said that it was his son's birthday and he could not come. Bockris tried two other colleagues, each of whom had an excuse not to come. Bockris realized that no one was interested in seeing the anomalous result.

Bockris recalled the eerie similarity between what was happening at Texas A&M on the matter of tritium—no one would come and look—and what is said to have happened in the early seventeenth century with Galileo and his telescope. The Church was then very much in control; its view was that the Moon was "queen of heaven and perfect," therefore, no need to look through a telescope to confirm what it already knew to be true. It did not want to be told that the lunar orb had imperfections. When informed by Galileo that much structure was evident on the Moon, the clerics turned away. They refused to look through the telescope. In four hundred years, the dynamics of intense paradigm shifts in science had not changed much.

**Deeper Heresy**

The work on tritium continued through that of Chien, Bockris, *et al.*, which was published in 1992.[4] In 1991 Bockris received a strange telephone call from a technician, who introduced himself as Joe Champion. This fateful call would lead to a new era of controversy, but also new discoveries at Texas A&M. Champion said that he had read about the Bockris work on tritium and wanted to demonstrate that he could initiate an excess heat reaction more quickly than the hundreds of hours which had previously been required. Champion claimed that he worked on the campus of a University in Tennessee, where he had a trailer containing his equipment. Champion promised that if Bockris could send someone to see his experiment, they would be convinced.

At that time Bockris had a very intelligent and able postdoc, Dr. Ramesh Kainthla. Also, Omo Velev, who had been working toward his Ph.D. in Sophia, Bulgaria, had come to work with

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Bockris. Bockris asked them to go together to see Champion's work in Tennessee. They returned to report that he had shown them the apparatus, given some instructions, and let them find out for themselves what it could do. They thought they had seen excess heat from the device within an hour of switching it on, so what he had promised appeared at first glance true, albeit some 30% less excess heat than he had promised. This seemed impressive, although the group was not told how he did it. Later, in March 1992, Champion called again to say that he had now found support money and would like to disclose more about his work.

When Champion first came to Texas A&M, he had an unusual appearance, Bockris recalls. He was tall and heavy-set, looking more like a football player than a scientist, yet he was very shy and diffident. He spoke with a slight stammer and told Bockris that he had been working for two or three years on the processes that he was to recount, but that he needed independent verification. He went on to describe the work, which was nothing less than heavy element transmutation at low energy—in effect, alchemy. Yet claims of alchemy, old or modern, bore some relation to the ongoing heresy of cold fusion, the production of helium from hydrogen under mild conditions. Bear in mind that by 1991, Dr. Melvin Miles of the U.S. Navy had confirmed that helium is created in excess heat-producing cold fusion experiments.[5]

The essence of Champion's approach was that one could calculate the frequencies of electromagnetic radiation that should be imposed on a material to make it undergo a nuclear transmutation to another element. He claimed that he worked only with materials, the nuclei of which had a quadrupole moment, for this brought them into the range of the types of frequencies which his device was said to produce. The nuclei would absorb energy provided by the magnetic and electric fields. If the amount of energy absorbed were great enough, transmutation would supposedly occur. He presented Bockris with a report, which included a number of calculations, none of which made any sense according to conventional understanding. Champion's claim was an even greater heresy than cold fusion, because it was much more difficult to understand how elements with nuclei of much higher positive charge (greater numbers of protons) than hydrogen could have their Coulomb barriers (electrical repulsion barriers) tunnelled through by charged particles.

Following the Champion visit, a Mr. William Telander arrived, who was apparently working in a financial capacity with Champion. Telander gave the impression of a genial, relaxed, wealthy Californian. His line was that he had inherited a restaurant chain from his mother. Telander said that he distrusted the United States as a safe place for his investments, because the government pried into everything. He said that he had taken to Europe the money he had gotten from the sale of the restaurant chain. He claimed various interests in Belgium, Germany, Russia, and China. He claimed he had an office in Switzerland. Bockris phoned that office to verify it and it did exist, but Bockris was told on two occasions, "Mr. Telander is traveling."

On Telander's first visit to Texas A&M, he offered $100,000 for the group to test Champion's unusual claims, but in the ensuing conversation Bockris got him to increase the offer to $200,000 to spend on whatever the group wanted within the general area of these "inorganic reactions." Telander said that he was intrigued by Champion's claims, which Champion said had been verified not only in Tennessee but in some work which he had done at the University of Guanajuato in Mexico.

Bockris phoned the scientist in Mexico with whom Champion said he had collaborated. A Professor Garcia gave a partial confirmation of what Champion had said. He had not really collaborated with Champion, but Champion had brought him samples which had been produced elsewhere. One set of samples was labeled "untreated" and the second "treated." The "treated" samples *did* contain some traces of gold and some other noble metals and were radioactive. However, he made the point that he had no idea where these samples had come from and whether the radioactivity was indeed due to some kind of process or had simply been put there. He seemed negative and hesitant about the whole thing; he made Bockris very suspicious.

When Telander finished his presentation, Bockris explained that he had become interested in this kind of work in the course of investigation of the Fleischmann-Pons work (*i.e.* tritium production) and would like to do it, even though it appeared a long-shot and extremely controversial. Bockris told him that the best way to fund the work was to approach the Development Foundation of Texas A&M and make a gift. The advantage of a gift was that the administrative overhead was then only 5%, a management fee, whereas if he went via the Research Foundation route, the university overhead would be 30-40%. There was a catch: by going to the Research Foundation, he could have a contract to carry out a definite program of research, whereas if he gave the University a gift, the University could determine what they wanted to do with the money.

It would be within the contract limits that the gift might not be used to fund his research at all. Bockris pointed out to him that in practice the gift path would be preferable. He could write an entirely legal letter to the University in which he donated the money, saying that it could be used by the University in whatever way it wanted. There would be a clause in which he could state that he would prefer the money to be used in the support of the work of Bockris. The University would not be likely to use the money except as desired by the donor, because it would want to encourage the donor to give more money in a second phase.

Bockris introduced Telander to the head of the Development Foundation on his second visit; Telander conferred with him. Bockris tactfully left them alone and was later told that the offer had been duly noted. Bockris would be told later whether it had been accepted by the University or not. He met with the Head of the Chemistry Department to tell him of the gift that would be coming and the fact that it was for strange, heretical work, which he outlined to the Head. Bockris thought that a general designation of "investigations into inorganic reactions" would be true, but discreet, yet would encompass everything.

The eventual authority who accepted the money was Dean Kemp. It took the University several weeks to consider Telander's offer. Although he was flying around in his private jet and wasn't often in College Station, Telander did visit the Bockris group on another occasion. He finally sent one of the lawyers, with whom he seemed to be in frequent contact, to ask officials whether they were going to accept the gift. Finally, official approval was given.

**Champion Begins**

The first reaction to Joseph Champion within the laboratory was that he was an oddball-type of inventor, not a person of any special scientific training. (Earlier, he had run a laboratory for calibrating test instruments, skills perhaps acquired during his military service.) Telander had sent a large quantity of specialized electronic equipment to accompany Champion, which was promptly moved into the laboratory. Discussions with Champion revealed that he needed an electrochemical cell to couple with his electronics. The lab had many such cells, so he was provided with one, plus ancillary equipment. Champion connected his device and proceeded to carry out experiments. The device produced pulses of a bandwidth and frequency which he could control, including a "beat frequency" mode. Champion had a

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list of quadrupole moments of certain elements and charts of other characteristics of nuclei, all in a computerized data base. For a given nucleus he could examine its properties to find the "appropriate frequencies"—those he thought would interact with the quadrupole moments of the targeted nuclei.

Champion set to work with a solution of ions, which he said he would transmute. Very quickly Bockris and his students got the impression that Champion was trying ideas that he had not examined before! It was unsettling, but who knew what a clever, intuitive tinkerer might come up with when so many other strange nuclear anomalies had already emerged in the "mainstream" cold fusion field itself.

This first phase of Champion's work at the Bockris lab lasted about six weeks. The Bockris team had become extremely skeptical that this was going to result in anything useful, so it left Champion entirely alone in the laboratory. In fact, they treated him as a postdoc—he was registered at the University as a "guest worker." Occasionally the group thought there were signs of success. Some solids did seem to be deposited and were subject to X-ray and other kinds of analysis. There was a hint of an anomalous production of gold, but the experiment wouldn't repeat, so the group gave it up. In view of what happened later, it is very important to note that in this period of unsuccessful work, *Champion had complete freedom to cheat if he had wanted to*. The group of academic scientists had little control over what Champion did at that time.

Telander was paying Champion's living expenses at a local hotel. Champion was risking his livelihood in admitting the failure of his work up to that point. He didn't know whether Telander would dismiss him on the spot and go off elsewhere. In fact, he retained Telander's interest by saying that he had used "another method" to carry out the work which had proved successful at the University of Guanajuato in Mexico. He called the new method "the explosion method." Bockris would later call it the "impact method," because a Russian group in 1998 had claimed to find nuclear changes occurring after it had subjected its samples to explosions.[6]

The group went ahead with Champion's impact method, because Telander had asked that this be independently verified. Postdoctoral students, Dr. Lin and Dr. Bhardwaj, were to work half-time on this. In practice, they would work for three to four weeks on the Champion work and then go back to their own research activities (on which they were getting one-half salary) for three to four weeks.

A rough outline of the impact method: There were initial starting mixtures designated by Champion, which typically contained inexpensive materials, such as lead chloride and mercurous chloride, together with carbon powder and potassium nitrate. Sometimes other chemicals were added, such as sulphur and silica, but the carbon powder, potassium nitrate, and the cheap metal chlorides were always present.

The mixture was put into a large coffee grounds can, and this was placed in a protective crucible, all within a fume hood, and ignited. This was done with a remote igniter, leading to a muffled explosion and dense fumes (sometimes from a sulphur constituent), which were removed by the ventilating hood. These were, in effect, low-level pyrotechnic gunpowder explosions. After a "burn," Bockris would approach the crucible and peer in just after the explosion. Much earlier in his career, he had had experience with high temperature optical pyrometry. It seemed to him that the color of the mixture in this pot just after explosion indicated a temperature that might approach 1,000°C, but would certainly be over 800°C. It is clear that this transmutation methodology has its origin in classical alchemy—from "recipes" that are hundreds of years old.

According to Champion's instructions, which allegedly came from his earlier work in Mexico, the post-burn crucible had to be left for two or three days before it should be analyzed. During this waiting period, the researchers applied a Geiger counter to the mixture and there seemed little doubt about it in their minds: radiation, apparently from radioactive materials in the residue, was present, which had not been there prior to the ignition. But these measurements were crude. They simply held a Geiger counter at a fixed distance from the crucible and took the count at intervals over twenty-four hours. It is important to define the set up. Telander had insisted that one of the offices in the corridor be occupied by Champion or by a secretary. There was also a lawyer, who was present sporadically, there presumably to initiate patent claims if a positive result were to be obtained.

The experiments were lengthy and tedious. The carbon had to be ground fine; other materials had to be obtained and ground up; and all had to be mixed for three days. The actual impact experiment itself, which occurred with a "woosh" sound, was over in a few minutes. The mixture would be cooled for three days, during which nothing could be done except measure the radioactivity to determine whether a characteristic time variation existed that might help identify the radioactive species. An exciting variation in the radiation counts was observed in the early days of the Bhardwaj-Lin experiments. The group plotted the declining Geiger counter readings (counts per second) as a function of time. The logarithm of the count was linearly proportional to the elapsed time after the ignition—the very behavior one would get in the decay of a radioactive isotope! The half life measured corresponded to that of platinum-197 (18.3 hours). This had been predicted by Champion earlier. He said that Pt-197 was an intermediate in going from mercury to gold. This seemed interesting, though it wasn't clear to Bockris why mercury, element 80, should become platinum first (element 78), and then onto gold, element 79. But the group was eager to see something measurable, so this ostensibly positive result heightened enthusiasm. There might be ways of explaining away enhanced radiation after a burn, such as a concentration effect of naturally occurring radioisotopes in the condensed ash. But a time-variation, indicative of a decay rate, was something else. The group later published the astonishing result.[7]

After the first runs had been carried out by Bhardwaj and Lin, the group had to analyze the material which Champion claimed would now contain noble metals. Bockris was extremely anxious to do this in such a way that it could not be faked. He didn't want Champion or anyone outside his research group to have any hand in it. Bockris therefore packaged some of the material himself and sent it to four analysts: to some friends in Australia, in the Government Research Organization there; one to a Canadian analytical organization; one to an organization the group had identified in Nevada, which specialized in analyzing mineral deposits; and one sample was kept at the University, to be examined by atomic absorption spectroscopy and an analysis offered by the local nuclear reactor staff.

The results of the first run were disappointing. One had to take into account that a considerable amount of material was expelled in the explosion, so the weight of the initial material in the crucible had to be measured and then the weight after the explosion. Finally, the concentration of any noble metals (analyzed in different ways by the various companies) had to be expressed as a fraction of the initial mass of material. The results of the first experiment showed a negligible change from start to end point, *i.e.* the experiment did *not* verify Champion's claims. Failure of this first experiment, using the method said to have been verified in Mexico, hurt Champion's credibility. The group tried again.

Champion's role in all this was that of an advisor. He talked to Bhardwaj and Lin freely and there were frequent conferences in Bockris' office during which detailed discussions of the exper-

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imental methods took place. The team carried out several experiments successively over the course of April, May, and June 1992. Remarkable results were observed, which all regarded as being very controversial. The group had found noble metals present, just as Champion had predicted! The general characteristics of these results, according to Bockris, were as follows:

1) The new metals found were gold, ruthenium, rhodium, and platinum. Gold was always dominant and its maximum concentration found was about 300 ppm. Other materials were in lower concentration, around 10 ppm and sometimes less than this, but above the error limits of the methods (about 1 to 2 ppm). The group counted these as significant. Each experiment took three to four weeks, including the time to send materials for analysis. The three successful runs occurred from April through June.

2) The analysis by the various analytical organizations were not always in good agreement; sometimes there were differences of as much as 50%. But qualitatively there was no doubt that in the three experiments using Champion's impact method, noble metals appeared to be produced. There always was a before and after concentration measured by the analytical people, so it seemed that the basic result, production of over 100 ppm of gold and lesser amounts of other noble metals, was secure.

3) The best analysis, in detail and thoroughness, was carried out by the National Institute of Metallurgy in South Africa. The organization might have been expected to obtain the most reliable result, because of the importance of noble metal deposits—particularly gold and platinum—in the South African economy. The National Institute of Metallurgy in Johannesburg was used to dealing with such analysis; they provided two methods of analysis, both of which worked out to give about the same result.

To Bockris' amazement, when Telander heard about this, he was *not* pleased! He was totally unaware of the anomalous nature of the results. Although he had come to Bockris with the attitude that he was a disinterested wealthy man who would like to find out if there was truth in an unlikely claim, he rapidly became a very interested businessman when the group reported that noble metals could be produced. He was dissatisfied: 100 ppm is about 0.01% of the mixture and it would only have satisfied Telander had they been able to produce actual *visible* pieces of metal. On some occasions the group could, in fact, see tiny specks of something gold in color, which did turn out upon analysis to be actual gold, but the amount of these yellowish specks must have been in the milligram range. This attitude of Telander was completely unscientific, focusing on the practicalities of future gold production from the method, while ignoring the astonishing evidence that gold might have been produced at all.

By August 1992 Telander abruptly announced that he did not want to continue the work at Texas A&M, because of the ridiculously small amounts of noble metals the group was obtaining. He said that he would move to a commercial laboratory in Chicago and there the work would be done on a "proper scale." This made no difference to the $200,000 he had given to the University. Bockris was able to continue using it in other research projects. In September 1992, Champion left the Bockris laboratory with a positive feeling. He had come in April 1992 and left in September. Although there had been ups and downs, particularly the failure of the electromagnetic method, his claims appeared to have been verified, although the amounts of noble metals obtained were miniscule.

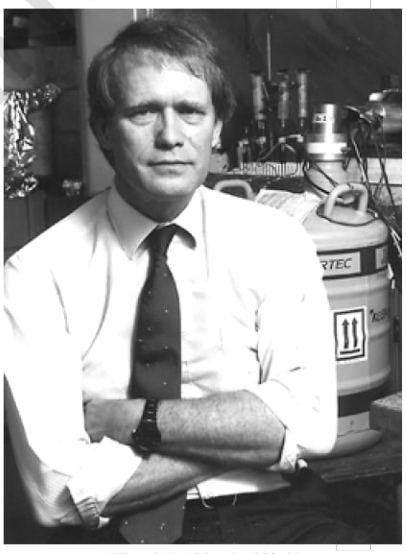

Photo courtesy Texas A&M.

The late Kevin Wolf at the Cyclotron Facility.

Despite the very dubious nature of Champion's testimony, Bockris' results seemed to be sound enough. There were Russian researchers who reported in Vancouver in 1998 at ICCF7 that it had used the "impact method" and found an altered ratio of the isotopes in cesium—if true, clearly a nuclear change. Then there is the extensive work by Dr. Tadahiko Mizuno at Hokkaido University, in which numerous transmuted heavy elements appear—including gold—in carefully measured electrochemical experiments.[8]

**Kevin Wolf's Alchemy Nightmare**

The transmutation results obtained by Bhardwaj and Lin, from the "recipe" given by Champion (however he had mysteriously obtained it from the "alchemy underground" of shady "adepts"), were obtained between June and August 1992. In October 1992, at the Third International Conference on Cold Fusion (ICCF3) in Nagoya, Japan, rumors circulated that Kevin Wolf had obtained remarkable heavy-element transmutation results in cold fusion experiments that had been conducted *covertly* using Fleischmann-Pons-type cells! The reported transmutation findings were completely serendipitous. If the rumors were true, a *second* front of modern-day alchemy had opened up at Texas A&M. Yet the Wolf transmutation results would not see the light of day until April 1995.

At ICCF5, which met in Monte Carlo, Monaco (April 1995), EPRI's Dr. Tom Passell revealed for the first time the results of the EPRI-funded work of Kevin Wolf, which had led to the transmutation findings. There it was: unambiguous evidence of the transmutation of heavy elements by some heretofore unknown nuclear process occurring in Pons-Fleischmann-type cells.

Why had Wolf wanted to cover up such a major discovery? (In fact, he met an untimely death in 1997 without ever having published a paper about them.) Recall that physicist Wolf was initially a pioneer in cold fusion, who had made announcements in the spring of 1989 about his detection of low level neutrons and tritium. It seems that Wolf had lost his nerve after the scurrilous attacks by Gary Taubes in 1990. Though Wolf had played an ambiguous role in the attacks by Taubes against Bockris, Wolf had concluded in the spring of 1990 that his own tritium results were flawed. They were most likely the result of pre-existing contamination of his palladium, he said. He "withdrew" these results in the general press, *e.g. The Wall Street Journal*, but never issued a formal retraction to any scientific journal, as far as is known—other than his negative remark to reporter Robert Pool in *Science*.[3] Thereafter, he became a quiet skeptic of the cold fusion field, even though he continued to be funded by one of Dr. Passell's colleagues at EPRI, skeptic Dr. Tom Schneider, who was bent on tearing down cold fusion. Wolf continued his electrochemical cold fusion experiments, continuing to find low-level neutrons (and *not* publishing these either), but nothing else of interest.

In 1992, Dr. Wolf made what to him must have been a nightmarish discovery while chasing down low-level neutrons. Three of his palladium cathodes that had undergone Pons-Fleischmann electrolysis in heavy water were found in routine Geiger counter examination of the lab to be highly radioactive! Subsequently, the cathodes were examined in sophisticated gamma-ray spectrometers at various laborato-

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ries (including Los Alamos National Laboratory), which could observe the intensity and frequencies of the gamma ray emission lines. Experts, including Wolf, who saw the multiple spectral lines of gamma emission had no doubt that these were the signatures of radioactive isotopes with masses near that of palladium. The results were published by Dr. Passell in the ICCF5 Proceedings.[9]

It may be difficult to understand this, but Wolf could not imagine that these astonishing results—*in which he believed fully*, but could not thereafter reproduce—had anything to do with "cold fusion." He apparently died imagining that these radioactive isotopes might be due to the effects of cosmic rays penetrating the Earth's atmosphere—*which just happened to strike his "cold fusion" cathode*. He apparently believed that hypothetical Weakly Interacting Massive Particles (WIMPS)—the purported dark matter or "missing mass" of the universe—might have caused the transmutations. Wolf apparently wimped out, so to speak, on his own solid data. The world of science was prevented from hearing about this work at ICCF4 in December 1993. Wolf had been scheduled to talk at the ICCF4 conference in Maui, Hawaii. The program listed his talk as "To Be Announced," but Wolf was "encouraged not to attend" by cold fusion skeptic Tom Schneider, with the implied threat that his funding might be withdrawn if he went public.

Several weeks after a Pons-Fleischmann-type cold fusion experiment had ended in Wolf's Texas A&M lab, at least one cathode was found to be inexplicably radioactive. Gamma rays from at least seven radionuclides were observed. The number of counts observed per peak was on the order of $10^4$ to $10^6$ counts, with a high signal-to-noise ratio of about ten. The statistical significance of the data was high. Radionuclides of silver, rhodium, ruthenium, and palladium were detected. (See *Infinite Energy*, No. 2, p. 30, and Reference 9 for a more complete discussion.) Since there is no conventional explanation for how palladium can be made radioactive with this pattern of isotopes (not even in a nuclear reactor), it can be assumed that a "cold fusion" reaction was involved in some way. To this day, no one knows what could cause the bizarre Wolf data presented by Passell at ICCF5. In any event, it has been clear at least since 1992 that deuterium-fusion reactions are not the only participants in cold fusion phenomena.

The Wolf results had been obtained in September 1992, *after* the first impact method experiments in May in the Bockris group had shown new elements apparently forming under mild conditions from lead and mercury compounds. On returning from the 1992 Nagoya, Japan ICCF3 meeting, at which the Wolf results were only *rumored*, Bockris urged his co-workers to try the impact method again. He wanted them to redouble their efforts, because of Kevin Wolf's results.

New impact experiments resumed in December 1992. Much to the group's chagrin, the amounts of gold found were within the limits of error—a null result. During the Christmas vacation of 1992, about eight runs were carried out by Dr. Bhardwaj to try to recover the results that had been obtained in the summer, but no anomalous noble metals emerged. (See Exhibit A. Dr. Monti suggests that a "seasonal effect" described by ancient alchemists—results are only good from about March 25 to June 15—might have been responsible.) By February 1993, Bockris became convinced that the group had to withdraw the support it had given during the summer to the results of the impact method—there were too many doubts about it. Bockris wrote a letter to the lawyer with whom he had been most associated in dealings with donor Telander. He noted that the group could not repeat the results. Shortly after this, the work supported by Telander had to stop, allegedly because of difficulties Telander said he was having.

The Bockris group continued its transmutation research with other funds. It now worked on the electric arc, carbon-to-iron reaction with the help of Dr. Sundaresan of the Bhabha Atomic Research Center (BARC), in Bombay, India. A small amount of iron was apparently produced in the experiments. It was well above the level which corresponded to the tiny amounts of impurity iron remaining in the spectroscopically pure carbon electrodes. A dependence on oxygen ($O_2$) emerged; no iron was produced without oxygen. There was an attempt to hypothesize a nuclear transmutation that would be consistent with the excess heat evolved in the process.[10]

## Exhibit A: The Role of Dr. Roberto A. Monti

Dr. Roberto Monti, an astrophysicist based in Italy, had been developing a revisionist model of atomic and nuclear structure for years, based on his study of the historical development of chemistry and physics from the eighteenth century through the early twentieth century. When cold fusion was announced in Utah in 1989, Monti attended some of the earliest scientific meetings in Italy on the topic. Some of his initial reactions to the Utah claims are recorded in Italian newspapers of the time; these, in turn, were quoted by U.S. papers. So John Bockris had been in occasional scientific communication with Monti, prior to the arrival of Joe Champion and Telander. When the "Philadelphia Project" began, Bockris thought it would be useful to have Dr. Monti lend his expertise in monitoring the heretical alchemical studies at Texas A&M. Monti was a silent observer of the happenings among Bockris, his students, Champion, and Telander. Monti noted both the apparent successes and failures of the experiments and learned from them. As I have discovered from extensive discussions with Dr. Monti, the history of atomic theory, classical alchemy, and the happenings at Texas A&M are very rich with interconnections, strange conflicts, and colorful happenings.

Since 1993, Monti has continued to conduct transmutation experiments of his own based on his "alpha extended model" of nuclear structure, which implicitly permits the kinds of low-energy ("cold fission/cold fusion") nuclear reactions that have been claimed by some scientists and "real" alchemists for centuries. He is now supported as the Director of Research of Monti America Corporation, a Vancouver, Canada-based company involved with low-energy transmutation. (Monti resides mainly in Italy.) He published extensive descriptions of his thermal-alchemy transmutation experiments in *The Journal of New Energy*, Vol.1, No. 1, 1996, p.119 and in the same journal, Vol. 1, No. 3, 1996, p. 131.) At ICCF8, his poster abstract claimed continuing progress in experiments that show the feasibility remediating nuclear waste:

**Nuclear Transmutation Process of Uranium**

The possibility to cause nuclear transmutation of stable isotopes by means of ordinary chemical reactions suggested the possibility to cause nuclear transmutation of unstable isotopes. A first series of experimental tests was made from 1993 to 1995 with positive results. A new series of independent tests has been performed at ENEA laboratories, starting October 1997 up to April 1, 1998. The results of the first and second series of independent test were reported in ICCF7 (Vancouver 1998). A third series of independent tests was made in the same laboratory (ENEA, Saluggia) on May 21, 1998 and May 25, 1998, using uranium nitrate, again with positive results. A new series of independent tests will be performed in an independent laboratory in the USA in April 2000. The results of the experiments made in May 1998 and in the new ones of April 2000 will be reported in ICCF8 (Lerici, May 2000).

Experimental reports provided by Monti describe tests in which gram-level destruction of radioactive material occurs. We plan to follow up on the Monti story in future issues of *Infinite Energy*.

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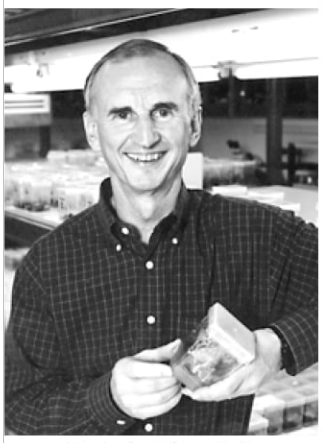
For another seven months, the group continued its low-energy nuclear reactions work. Then unexpectedly, an inflammatory letter appeared in the local newspaper, the *Bryan-College Station Eagle*. It was written by Dawn Wakefield, a former student of Bockris. Although the group had not done any transmutation work involving the impact method for six months (the approach that resembled classical alchemy), Wakefield accused it of the "heinous crime" of performing medieval alchemy at a state university. The letter stirred a hornet's nest of other troubles.

A call came from Joseph Weiss, a reporter with the *Dallas Morning News*. Weiss acknowledged that Dawn Wakefield's letter had led to his inquiry. He knew of Joe Champion and the gift of $200,000, the final disbursements of which had been interrupted by actions of the California Securities and Exchange Commission. Weiss wanted an interview. Bockris could see trouble coming, so he consulted the Chemistry Department Head, who obtained a recommendation at the TAMU Vice Presidential level that Bockris should grant the interview.

The interview was held on a Saturday morning. To Bockris' surprise, shortly after the meeting began, Dean Kemp, whom he had never met before, entered his office and said that he wanted to be present. Kemp had apparently heard of the interview from the Office of University Relations, whose representative was also present. Dean Kemp, indeed, had a personal interest in the *Dallas Morning News* story, for it was he who had approved the grant from Telander. Telander had told everyone that the donation had come from his personal funds, which originated with the sale of a restaurant chain inherited from his mother. Telander consistently claimed this, and it may be true, despite other disturbing news that emerged. California securities authorities had formally accused Telander of misappropriating $11 million from investors. Telander had apparently accepted funds from investors for investments in Switzerland, where arbitrage schemes on currency fluctuations allow risky but high rates of return. When questioned, Telander argued that he had merely extended their investment gamble in backing a development, which, if successful, would bring even higher returns. But Telander had not gotten his investors' approval for this, and there was also the discrepancy between the amount given to Texas A&M and the amount authorities deemed misappropriated. Telander claimed he had spent millions of dollars at other labs to follow up the results obtained in the summer of 1992 at Texas A&M. Bockris had no evidence that this was true.

Bockris was very frank with journalist Weiss. The discussion was recorded on four audio tape machines: one owned by Bockris, one by Weiss, one by a representative of University Relations, and one belonging to Dean Kemp. The interview lasted several hours, two hours before lunch and at least one hour after lunch. Mr. Weiss had a lot to write about, for Bockris had no reservations in telling him everything he knew about the entire business of the funding, the scientific work that the group had done, the results it had obtained, etc. He gave it to Weiss "straight," pointing out that no one understood the mechanism of the impact method, which had seemed to produce tiny amounts of noble metals. Bockris emphasized that the work sprang out of his verified and published work on the tritium-producing cold fusion reaction. He stated that he had wanted to see if a similar kind of nuclear reaction "in the cold," obtained with hydrogen isotopes, might also be found with elements of higher atomic number. Bockris told Weiss that the results had been disappointing, because after the promising experiments, the group found that it could not repeat the results, although some new anomalous radioactivity had again been observed.

Shortly after the interview, Bockris was astounded to get a letter from Dr. Robert Kennedy, Vice President in charge of research at Texas A&M. Kennedy said that Dean Michael Kemp had accused him of "misconduct of research." It seemed that Kemp read into the interview things which Bockris had never meant. During the heady times after the group had gotten good results, but before it tried to replicate them, Lin and Bockris had been invited by Telander to go to Mexico City to make a presentation about the work to a group of science journalists. Bockris was pleased to do this and both Lin and he spoke for perhaps five minutes each about the research. Bockris pointed out something which has now been widely verified: that if transmutation in the cold were indeed true, there would have to be a major revision in the theory of nuclear chemistry.



Dr. Robert Kennedy
*Photo courtesy Texas A&M University. Photo by Jean Wulfson.*

Dean Kemp read this statement quite differently. He thought that Bockris had gone to Mexico as an advocate of sponsor Telander. Kemp believed that Telander wanted to commercialize the products of the group's findings. Since transmutation in the cold was impossible, thought Kemp, any supportive statement that it could occur, or might have occurred must also be fraudulent, hence constituting misconduct of research. In his view, Telander wished to deceive the Mexicans and sell a process which was nonsensical. Kemp

## Exhibit B

**The Failed Petition at Texas A&M to Demote Bockris (This was signed in 1994 by 23 out of 32 Distinguished Professors at Texas A&M.)**

A Request

Professor John O'M. Bockris' activities since 1989 (the inception of the "cold fusion" embroglio), and particularly recent allegations that he lent his name and that of our university to the fraudulent scheme to promote a bogus engineering enterprise, has brought this university into disrepute. Note that on page 6 of the "Policies and Procedures Regarding Distinguished Professor Appointments" (September, 1993) it is stated that "The Distinguished Professors. . .bring honor and recognition to the University. . ." Instead, we that believe that Bockris' recent activities has made the terms "Texas A&M" and "Aggie" objects of derisive laughter throughout the world among scientists and engineers, not to mention a large segment of the lay public. The "Alchemy" caper is, everywhere, a sure trigger for sniggering at our university. And so it should be. For a trained scientist to claim, or support anyone's claim, to have transmuted elements is difficult for us to believe and is no more acceptable than to claim to have invented a gravity shield, revived the dead or be mining green cheese on the moon. We believe it is sheer nonsense, and, in our opinion, could not have been done innocently by one with a lifetime of experience in one of the physical sciences.

In view of the above consideration, we the undersigned Distinguished Professors of Texas A&M University hereby request the Provost to take steps to revoke the title of Distinguished Professor now carried by John O'M. Bockris. We do this because of our belief that Dr. Bockris' alleged disregard of the accepted standards of scholarly and professional behavior has brought great embarrassment upon this university and his colleagues. In our opinion he no longer merits the title of Distinguished Professor.

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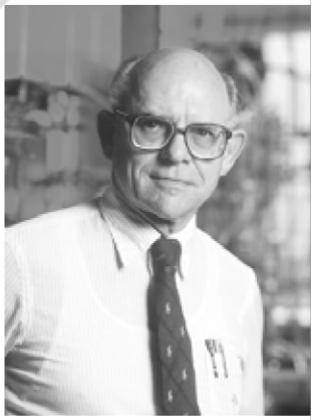
thought that it was a travesty that a Distinguished Professor at Texas A&M University would support a man like Telander.

The disturbing accusation was backed up shortly afterwards by a remarkable document (see Exhibit B), which came from the group of other "Distinguished Professors"—twenty-three of them. "Distinguished Professor" is the highest title for a professor at Texas A&M University—all are world-famous in their respective fields. The purpose of the accompanying document was to suggest that anyone who was crazy enough to believe that tritium could come from deuterium reactions in the cold, and then go on to say that metals transmute to other metals, including gold, must be certifiably scientifically idiotic. Worse, it would be *prima facie* evidence of fraud—fraud for the sake of money.

A group of four of Bockris' peers was assembled, all Distinguished Professors. Bockris met them in the building of Texas A&M housing the office of the General Counsel. In the inquiry, Bockris gathered six of his collaborators, each of whom had had experience in either cold fusion or transmutation work. He wanted them to be on hand if questions of experimental design or handling were raised. Bockris had paid for the services of a lawyer after the accusation. Though the University hesitated at first, attorney Gaines West was allowed to accompany Bockris into the conference room in which the "trial" took place. Present also was the Assistant General Counsel Genevieve Stubbs, a vigorous and capable attorney who a few years earlier had assisted Bockris when the Taubes allegations appeared in *Science*.

Bockris first asked permission to make a ten minute presentation, in which he pointed to questions of legality in what the University had done. For one, the rules of the University Policies and Procedures Manual state that no one in the Administration may speak to reporters or give interviews regarding a Professor's work without his or her permission. A *Newsweek* magazine article had, in fact, quoted a spokesperson of the TAMU administration, ". . . the work on transmutation was embarrassing the University."[11]

Then Bockris summarized the work of his research group. He pointed out that the team had repeatedly generated tritium in electrolysis of heavy water, and that this was an indisputable "nuclear change in the cold," which had been published in refereed journals. Thus, he told the panel, it had not been unreasonable to examine similar behavior with heavier elements. Bockris related that against all odds the work seemed to succeed, but then after a pause of some months, the results could not be reproduced. Regarding questions about Joe Champion (who later turned out to have had an imbroglio with the law at an earlier stage on an unrelated matter) and William Telander, who was then under investigation for whether he had permission from his clients to invest 1% of their money in speculative research at Texas A&M, Bockris could only say that he knew nothing of any improprieties by Champion or Telander while he collaborated with them. In any event, whatever the ethics of Champion or Telander, this didn't seem to affect the work that had been carried out by Bhardwaj and Lin. [In a Los Angeles court in 1994, William L. Telander plead guilty to four counts of securities fraud and two counts of tax evasion. He had stolen $11 million from 380 investors, for which he served time in prison. In 1993, Joe Champion went to prison in Arizona on charges not directly related to the fraud charges that put Telander in jail. Later in the 1990s Champion returned to prison in Arizona (reportedly for a parole violation) and may still be there in 2000. See "Cold Fusion and Modern Alchemy" in *IE*, No. 15/16, p. 95, on the further travels of Joe Champion and hot fusion physicist Dr. Barry Merriman, who spent a significant effort in an attempt to verify Champion's later claims.]



Distinguished Prof. F.A. Cotton, a signer and major promoter of the Petition.
*Photo courtesy Texas A&M University.*

The four distinguished professors who were "trying" Bockris were pleasant, which encouraged Bockris and his attorney. There was no need to call in any of the six postdocs who had carried out the research. After only a week or so, the best possible outcome happened: Bockris was given a "complete exoneration" from the charges in a letter dated January 31, 1994.

The Distinguished Professors who had tried Bockris gave an account of their investigation: They had examined more than 1,000 pages of documents. They had obtained evidence from four or five people. (Dr. Wakefield, the initiator, had been asked to provide evidence, but had refused.) One of the exculpatory pieces of evidence cited was a note, hand-written by Bockris from a hotel in New York City. It was a draft of what had presumably been made into a typed letter later. It contained a specific warning from Bockris to Telander that he must not in any way use the successful results obtained in the summer of 1992 to imply that there might be some commercial value in it. This was a key point in the defense. After all, the accusation had been that Bockris had encouraged Telander in fraudulent gold-making activity. But how had the investigators even found the note? Bockris had suspected that his office at Texas A&M had been under surveillance for a long time and that various documents had been stolen, presumably by unauthorized entry at night. Apparent thefts of certain documents also occurred, apparently via unauthorized entry at his rustic home office. Who was paying the possible "private investigators" carrying out these intrusions? He never found out.

After the trial, Bockris continued with his research and teaching and had another four or five months of peace and quiet, just as he experienced after the end of the work supported by the dubious Mr. Telander. Unfortunately, news of a "new inquiry" erupted around June 1994. An article in the *Eagle* implied that the "new inquiry" had been set up to see if any "personnel changes" were needed as a result of the "Philadelphia Project"—the informal code name the group had given to the Champion-Telander work. How could this be, after the letter of complete exoneration? Bockris understood that a big initiative of some kind was underway and that decisions in secret "political trials" are not necessarily made according to the truth, rather according to the power exerted. Bockris had enemies at Texas A&M, and perhaps beyond, who were not satisfied by his exoneration.

The new committee came to be known as the "Ad Hoc Committee." When Bockris' lawyer inquired of the Assistant General Counsel what was the objective of the inquiry, he was told only that the University could investigate whomever and whatever it liked. The invisible "inquiry" went on and on. After some months, Bockris wrote to the Committee, pointing out that it was he who knew more about the "Philadelphia Project" (the informal name for the work that was used within the Bockris group) than anyone. He suggested that they could shorten their investigative work by inviting him to one of their meetings to ply him with questions, the answers to which could later be checked.

Bockris would later learn from a member of the Ad Hoc Committee that the primary mover against him was a professor in the Inorganic Division of the Department of Chemistry—Distinguished Professor F.A. Cotton. At a meeting with the Dean of Science, this ambitious professor had pointed out that he had published more than 1,000 papers, whereas Bockris had pub-

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lished only 700—a rather tenuous measure of relative rectitude.

Bockris received no reply to his letter asking to be questioned. Christmas 1994 was approaching. Bockris thought it would be a good idea to speak to the Chairman of the Committee, Dr. Robert Kennedy, with whom he had a good relationship in the past. Over the phone Kennedy was reluctant to speak, but he finally agreed to meet. Yet when they met he couldn't tell Bockris anything! He said that the Committee was doing its work. When Bockris asked him what the result would be, he said he didn't know, but that personally he was fed up with it all. He said cryptically: "There is a message from the Provost. He has asked me to tell you: 'Bockris will not be the only one.'"

The chilling message meant that they were thinking of firing Bockris, on what grounds he did not know. Any new charges had been kept secret from him— not exactly a model of ethical legal procedure. Bockris would soon learn from the leaky Committee that a principal reason for renewed investigation was that he had obtained results that were simply "impossible" and a cause for the University to be the object of ridicule. Bockris decided to spend a few thousand dollars more on the attorney who had helped bring the first inquiry to "total exoneration." The two agreed that the best approach might be to take the whole thing to the American Association of University Professors (AAUP).

An eleven page document was prepared,[12] which described what Texas A&M had been doing to Bockris since 1993. There had now been two years of continued persecution, a "trial," the exoneration, then the "new committee," the eleven months of investigation, the refusal to tell Bockris what any charges were, etc. The AAUP is regarded as a powerful body within the university community of the United States. It investigates what it considers to be unjust treatment of professors and it can punish a university if it finds cause. If a university is blackballed by the AAUP, new faculty of high quality will be less easy to hire. Texas A&M had good reason to be concerned about this: in the 1980s it had been under a cloud from the AAUP. Perhaps the University would not want to again risk censure. The Texas representative of the AAUP said that the Association might well send a team of investigators to Texas A&M to find out just what the university was trying to do to Bockris.

Though there is no proof that what happened next had anything to do with Bockris' letter to the AAUP, in the light of the chilling message given him a few months earlier, Bockris was both surprised and relieved that on May 5, 1995 he received a welcome letter from Acting Provost Charles Lee. It said that the eleven month investigation had shown that in no case had Bockris done *anything* that contravened the Rules and Regulations Manual of the University. It was tantamount to another complete exoneration, although the letter was not as warm as that from the first group of professors. It was written with a regretful tone, implying that there were no technical or legal grounds on which to convict him, "but . . ."

### The Shunning

One of the most difficult aspects of the treatment to which Bockris was subjected was social ostracism, starting with Dean Kemp's accusation and not even ending with the second exoneration. There were about sixty-five professors in the large Chemistry Department at Texas A&M. Most ignored Bockris for much of the two-year period in which the University, egged-on by ring-leaders in the Department, acted against him. After the first complete exoneration, two professors did congratulate him, but he was isolated. Bockris' wife Lilli felt it perhaps more than he, because she had a number of faculty wives whom she had known as friends. When she met them now in the supermarket, instead of having the usual kindly chat, they turned their backs on her. Lilli recalls that the year she spent in Vienna after the Nazis took over seemed to her less unpleasant and threatening than the isolation and nastiness which she felt in College Station, Texas from 1993 through 1995.

One would have thought that after all that had been done, everything would be settled now. This was not the attitude of many of Bockris' colleagues. The motivating force for the antipathy may be the subconscious fear that the discoveries of the Bockris group might eventually be proved and recognized. Then his original contributions would be rated as discoveries of great magnitude. There were at least two professors in the Chemistry Department who had made it known that that they expected to receive the Nobel Prize in Chemistry some day. The possibility that it might go instead to a colleague whose work they so much denigrated must have been an unwelcome thought. (They did not have the attitude of physicist Richard Feynman, who was displeased by the artificial focus on one person's accomplishment that the Nobel Prize system encouraged.)

Having failed in the three official investigations that had been carried out against Bockris, they decided that all they could do would be to persuade the head of the department to have Bockris shunned—as in an excommunication for religious heresy. No one was supposed to speak with the errant Bockris. For a long time, absorbed in his work as ever, he didn't understand that shunning was underway. Most of the colleagues had been ignoring him anyway since the inquiries had begun in 1993. He did notice, however, that whenever he wanted to talk to the Head of the Department, perhaps once every few months, he came to his office and did not invite Bockris to come to his. Of course, he was more than twenty years younger than Bockris, but later Bockris realized that this was an example of the shunning. The Head did not want anyone to see that he was talking collegially with Bockris!

Bockris' colleagues in the physical chemistry division took no notice of the shunning order, which might have gone around unofficially. In practice, the shunning made no effective difference to how Bockris carried out his work, though it was a very considerable act of spite. It proved once again that at least in the Chemistry Department at Texas A&M University, research results which do not agree with existing theory are not tolerated.

### More Intolerance

In 1995, an energetic young student in Engineering, Todd Hathaway, wanted to have a symposium on new energy sources. He was headed for service with the U.S. Navy, interested in serving in nuclear submarines, perhaps eventually to become a Captain. Hathaway had the characteristics of a leader, Bockris thought. He was polite, but also quite domineering and a bit arrogant. He persuaded the Head of his Department, via a committee of students who recommended seminars, to agree to a symposium on new energy sources. They invited four people to speak, of whom Bockris was the one who would talk about cold fusion.

The posters for the New Energy Sources Symposium caused a sharp reaction from certain professors, who apparently wanted to show that it would be impossible for Bockris to speak, but the line was that the entire symposium should be cancelled. Bockris learned that the movement to ban the New Energy Sources Symposium came from Chemistry. The quasi-official line was that "the speakers were not of good quality." The Head of Hathaway's Engineering department told him that there would be no lecture theaters available in Engineering for a symposium of this kind. Furthermore, if he tried to hold it elsewhere in the University, he would be held responsible for the consequences. He was later accused of having improperly used state property, just for having communicated with the speakers using an office typewriter! The *Eagle* newspaper treated the matter as though it cast doubt on the student and the proposed symposium.

Fortunately, Hathaway was not so easily put off. He went off

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campus to a local Catholic Church, to Father Sis, with whom Bockris had had occasional theological discussions. The church immediately gave him a room for the symposium, which was held with the original speakers.

The *Eagle*, which had so denigrated Bockris's experimental work on transmutation, sent a photographer to the symposium. This was presumably to ridicule it and perhaps show, if possible, that few students would turn up. The photographer went away empty-handed, however. The symposium took place perfectly normally with about thirty-five people present, including several members of the Chemistry Department. All went well with the four lectures and a discussion period. The *Eagle* fell silent. University censorship of new ideas may have been thwarted, but it was yet another example of how Texas A&M didn't really know how to deal with ground-breaking research that threatens the paradigm, contrary to the official line about "openness" in many Texas A&M publications.

**International Meetings on Transmutation**

After the Bockris group obtained the results in the work of Sundaresan (carbon-to-iron) and the results of Minevski (protons plus palladium to numerous new metals within palladium electrodes), they wondered whether others were claiming to have results parallel to theirs that nuclear reactions did occur "in the cold" within solids. It was Dr. Lin who suggested to Bockris that they should hold an international symposium on these matters. So Bockris went to Dr. Emile Schweikert, Head of the Department of Chemistry, to asked him for permission to have a one-day symposium held at Texas A&M. He replied, "Of course."

After much organizational activity, mostly by Dr. Lin, the symposium was held on June 19, 1995. (See *IE*, No. 3, p. 8.) and attracted about eighty-five people, including one from Russia and several from other countries. A student interpreter assisted the Russian scientist. The symposium went very well, beginning with Dr. Tom Passell of EPRI speaking about the hidden, controversial transmutation results of the late Kevin Wolf. It was the high point of the symposium, and certainly set the theme, for by itself this work proved that transmutation in the cold did occur in some metal systems.

The rest of the symposium went well. Tom Ward, an independent spirit from the DOE, was there. He spoke at the end of the symposium, praising it and saying that DOE money might well be available for such efforts "very soon." However, the funding never came.

A most unfortunate incident confirmed the worst fears about Texas A&M University and those who opposed the publication of new scientific findings. Doherty-Welch Distinguished Professor of Chemistry F. A. Cotton approached the meeting room with two colleagues in the early afternoon. Dr. Ward and another speaker were outside the lecture hall. When Cotton realized what was being discussed in the lecture theater, in a loud voice he shouted that these people were "all gooks." Ward took great exception to this behavior. He wrote a letter of protest to the Texas A&M President, pointing out that he had come to hear science, not to be insulted by a man who apparently could not bear to hear about this developing field.

This symposium was immediately discussed by the *Eagle*, which talked about ruffled feathers at Texas A&M. The article implied that there was something nefarious about the meeting. Bockris was just about to leave for Australia for a three-month period, but managed to write a letter to the *Eagle* about what Prof. Cotton was reported to have said. Bockris' letter was not published. Its main point was that the results of a large number of scientists from various countries were being reported at the symposium; that the process of science was to listen and to accept or reject these experimental results, to see where they might lead in possible revisions to theory.

In 1996 it was time to consider whether a second symposium should be held. Requests were sent out for speakers, with an encouraging response—about one hundred people registered for the symposium. Bockris approached the Head of the Department again and asked him whether they could hold the symposium at Texas A&M University as in 1995. This time, however, the Head had been told that he must submit any such request to a Committee which had been formed. The Committee, consisting of about a dozen members of the Department, listened to Bockris' five-minute presentation about the proposed symposium. Bockris circulated a review that had just been published by Ed Storms, which contained 468 references on research in cold fusion, a substantial number of them in refereed journals. Each member of the Committee had the review in hand the day before they were to be asked to agree to the symposium. Bockris received a memorandum the next day from the Head telling him that the votes had been unanimous to reject the symposium. It could not be held in the Chemistry Department. This was the standard of academic freedom at Texas A&M.

Bockris spoke over the telephone with one member of the Nuclear Chemistry group and asked him the reason for the unanimous vote against. The man replied, "They think it's a fraud or a joke." Ironically, the Cyclotron Institute at Texas A&M was devoted to transmutations, but at high-energy conditions in which particles had been accelerated. The equipment was valued in the millions, while what Bockris *et al*. had been using cost $10,000. Might this have something to do with the problems?

Another venue was found, the local Holiday Inn. Professor George Miley of the Nuclear Engineering Department at University of Illinois, a well-respected member of the nuclear community and editor of *Fusion Technology*, co-chaired the meeting with Bockris. Professor Joseph Natowitz attended, as he had the first symposium. He is the leading nuclear chemist at Texas A&M and Head of the Cyclotron Institute. There was plenty of time on the second day of the symposium for free-ranging discussion. Bockris asked Prof. Natowitz publicly: "Were you Kevin Wolf's boss?" He replied affirmatively. "Why then have you allowed Wolf's transmutation results of 1992 to remain unpublished by the Cyclotron Institute for four years?" He explained that the results "were not reproducible."

The two symposia on transmutation at Texas A&M University might have been a turning point in the attitude of many toward such reactions, although they are certainly not accepted by the majority of chemists even in the year 2000. But at the same time the American Nuclear Society has for three successive years held Low-Energy Nuclear Reactions (LENR) sessions at their national meetings. So the meetings which took place at Texas A&M would no longer be required; they were meant simply to start the introduction of the subject into the mainstream and were successful in doing that.

**Packham's Oral Examination: Aftermath of the Tritium-in-the-Cold Discovery**

It came to pass in 1992 that Nigel Packham, who had been the main worker in the initial research on electrochemical production of tritium, wrote his Ph.D. thesis. Packham had been working with Bockris for about two years on and off on cold fusion, but the subject in which he had begun with Bockris had been entirely different. It was aimed at examining the production of hydrogen from water using bacteria.

Packham's earlier background in England was partly in biochemistry, so the topic was ideal for him. His thesis consisted of two very different parts, one on hydrogen production from bacteria and the other on cold fusion.

Bockris suspected that bad problems might arise during the oral doctoral examination of Nigel Packham, because of the long-past article in *Science* by Taubes. So he arranged with the graduate school representative (who is present at all orals for the Ph.D. to assure fair play) to be prepared to remove the oral exam from the big lecture theater to Bockris' office. Bockris

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## Exhibit C
## "Excuse Me, I've Got a Meeting at 3:00"
### Brief Excerpts from an interview with Professor John Fackler at Texas A&M, November 25, 1996, conducted by Eugene Mallove

**Eugene Mallove**: For what period of time in 1989 would you say you still...had an open mind they might find something important, very significant? At what time did you perhaps begin to turn away from that view?

**John Fackler**: I suppose the time that I began to be less positive about the information fed to us with regard to whether it meant cold fusion...The time that I started getting concerned was the time when people started to fail to reproduce results.

**EM**: For example?

**JF**: Mark Wrighton [of MIT]. Mark worked very, very hard. I talked with Mark at length when he was doing it. I think Mark was probably more than anyone else quietly went about trying to reproduce the Pons-Fleischmann results. Went out to Utah himself, talked to Pons directly, attempted to set up the lab in a way that he could reproduce the results...It impressed me that Mark was probably the brightest of the people who were really after an effort to try to reproduce those results.

**EM**: And what else besides MIT, what other results do you remember...one that influenced you negatively?

**JF**: What influenced me negatively was John's results himself, that he had himself here. . .

**EM**: Which result was that?

**JF**: The tritium results. The initial tritium results. The work of Nigel Packham, the actual observation of the evidence for formation of tritium as observed by John and as published by John had a flaw in the publication process and I..

**EM**: What was that?

**JF**: The flaw of the publication process was that they had not calibrated their photolysis system for the formation of tritium. They were using...They were in a hurry...

**EM**: What I'm trying to establish, Dr. Fackler, is that at the time of the tritium first being seen, long before Taubes first came into view, was the spring of 1990. So we're back to the spring and summer of 1989. You began to have doubts at that point?

**JF**: I saw the results. I saw the spectral traces. I asked Nigel Packham and I asked John, "Have you gone back, not only to reproduce it..." I knew they couldn't reproduce it. That was the number one problem always, the lack of reproducibility. As a scientist, I don't believe anything until it's reproducible. Okay? That's how I've been conditioned to perform as a scientist. It's an interesting idea, an interesting concept; I don't believe it as factual until there is a defined way of reproducing the result...

**EM**: Taubes originally wanted to go to *Nature* with it, but *Nature* refused after their attorneys looked at it. So then he shopped it around and got it into *Science*. It was five pages and you saw it in advance. What was your reaction, do you remember, when you saw it in advance? I'm curious.

**JF**: If Gary had interviewed with me, I would have told him he has forgotten about the fact that inorganic chemistry has some very interesting aspects to this question of photoluminescence that were not taken into account in his article, not taken into account by Nigel Packham himself, and so Gary Taubes focused on the fact that John Bockris had tritium in the laboratory in his office, which he did. I saw it; I know he had that. So, he took the easy road out. Now, I would never accuse John of spiking it with tritium. . .

**EM**: ...Were you one of those signatories [of the petition against Bockris]?

**JF**: I'm sure I was one of the group that, in fact, asked that the administration revoke his Distinguished Professorship. Simply because he did, in my opinion, not follow proper scientific procedures and procedures particularly with regards to this press conference. He had been told administratively that he was not to be involved with any press conference activity with regards to transmutation. Prior to Labor Day [1992], actually.

**EM**: ...Was that your main reason or was it because you felt not only he was doing something that he was told not to do, but you didn't believe the result at all?

**JF**: Well, the results have never been produced in a way that are reproducible. Even to this day, and so I have never believed them. Right, that's correct.

**EM**: Let me ask you something about reproducibility because this is a very interesting point. There are certain phenomena, as you know, that are reproducible in the sense that they eventually get to be highly reproducible. But even then not totally, so they are in some sense sporadic, certainly when they're first discovered, such as in the case of a transistor. Okay, the effect was seen and when the people were trying to...when they regarded it as confirmed and it was announced and so forth, but subsequently in trying to make batches of this stuff...that would work...obviously they don't all come out well, you get defects, and some of them don't work. That's an instance...

**JF**: Why don't you use polywater as another example, which is the opposite result?

**EM**: What is your feeling about now there are two reports that came out from the university...

**JF**: What reports are you talking about?

**EM**: About the Bockris situation. One report of the first investigation exonerated him fully, it said. Then there was another report that Dean Curry gave me the other day and it also exonerates him with respect to the Philadelphia Project. It just says he's exonerated, okay. There is no punishment for him. There is no demotion of any sort, but there is basically a clean bill of health. What is your reaction to that?

**JF**: My reaction was very clear and I've stated that very strongly. That the committee that did the first report was given a very narrow charge and they—John Calhoun and John McDermott—are two people I talked to who were on that committee and they're hands were tied in terms of anything more than what they did. They clearly, from the evidence they were given and the charge that they were given, there was nothing further that should be done or could be done with regard to John. It wasn't an exoneration of the question of whether John had overstated the case publicly in Mexico City; that wasn't the issue. It wasn't an exoneration of John with regard to trying to convince his colleagues, like myself, that he had actually produced and reproduced, from the Champion results, gold—without testing whether or not the gold came from that source. That was not the questions that the Calhoun and McDermott committee were addressing. And so, consequently, the exoneration was obvious given those conditions, but my concern was John's failure first of all to deal with the issue properly of presenting scientific information to the public without convincing proof that that result was really correct and getting in bed, as a result of that, with people who were obviously making money on the process.

**EM**: So, people shouldn't make money over, say, data that isn't...

**JF**: I feel very strongly that if science is to be done that way, then we're in for trouble. I mean, it's easy to get money if you present to the public some ideas and creative concepts that are potentially lucrative, whether or not they're right or not.

**EM**: What about the opposite situation, where somebody has already a very lucrative program and they conduct research to defend that program...? [*Editor's Note: Mallove was considering the flawed work at the MIT Plasma Fusion Center against Fleischmann and Pons.*] Suppose that research, that money that's already been there, is based on data that isn't quite kosher. What then? I mean, if it's already an established program, then this program says, "Look, we're getting all this money" and then this other idea comes along and they shoot it down in defense of their own effort— partially in defense of their own effort.

**JF**: I don't know what you're driving at in this particular case in terms of...I mean, you do research to test ideas and you do research with a point of view to never until that idea is tested properly to be convinced that that idea is correct. You start from a non-convincing point of view and attempt to disprove what you're observing.

**EM**: Sure, but you certainly don't want to throw the baby out with the bath water.

**JF**: No, but it...No, that's true you don't. You want to try to deal with science in the world you're dealing with in an open fashion, but you have to be willing to accept every result that is pointing against your result and try to understand it and deal with it. I mean, there are a lot of cases in my own particular field where people have published garbage and it in some cases has cost the world a lot of money to demonstrate that it was garbage. Because the material looked alright. Everything was fine. In X-ray crystallography you can make mistakes. I mean, there was a time when people didn't believe you could make a crystallographic mistake, but that's been shown to be wrong.

**EM**: What do you think of Dr. Cotton, who apparently doesn't want to meet with me?

**JF**: You can meet with him in Paris.

**EM**: What?

**JF**: You can meet with him in Paris. He's there now.

**EM**: He has refused to. . .

**JF**: Well, sure, he read your side of the [cold fusion] story.

**EM**: And what does he think of it?

**JF**: He thinks it's not been established.

**EM**: He thinks it's not established? Let me ask you this... This was a letter that he wrote to *The Eagle* and I'm kind of curious what do you make of that? [**Editor's Note:** Texas A&M Prof. Cotton, in a letter published November 24, 1993 in *The Eagle*, among other negative remarks, states: "Bockris has repeatedly violated every known cannon of responsible scientific research...has perpetrated three of the most egregious examples of sick science ever seen...made claims that are completely irrational...capped a disgraceful career..."]

**JF**: I would say that Cotton has—and I've read this before—has a gift with words.

**EM**: Do you think he went too far in that letter to *The Eagle*?

**JF**: As far as I'm concerned, I couldn't write a letter like this because I don't have that talent for writing.

**EM**: Do you agree with the sentiments and the choice of language? I mean, let's face it, he's making various accusations against the colleague, Bockris. Do you feel that even though you couldn't write it because you don't have that talent with words, that this is an accurate and. . .?

**JF**: I can't say that because I can't reproduce those adjectives.

**EM**: Here it says...

**JF**: I've read it; I know what it says. But the point is that—trash, I've said trash.

**EM**: You've said trash?

**JF**: I've said that publicly.

**EM**: I'm curious, do you think cold fusion is trash today?

**JF**: I still have not seen anything that convinces me.

**EM**: Would you go over and talk to Dr. Wolf and look at his data, his transmutation data, and see that?

**JF**: At the moment...

**EM**: He did it in a Pons-Fleischmann cell.

**JF**: At the moment I am not particularly interested in that, but the point is that there are phenomena that do take place and you've talked about a couple that we don't understand and there may be phenomena here that we haven't completely understood.

**EM**: Absolutely. Here it is...[*Mallove shows him the published Wolf evidence.*]

**JF**: Whether or not it's a development of electrochemical energy in such a way that causes deuterium to fuse is a process that, in my opinion, I don't have any authority to talk about.

**EM**: Well, it turns out that the man over there has these gamma spectra—you can either accept them or not, but he's got them. He was one of those that rejected tritium, which was rejecting...

**JF**: Excuse me, I've got a meeting at 3:00.

**EM**: Sure. Well, basically, thank you for your time and I'm glad to get your perspective even though it's not mine, obviously. ❑❑❑

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feared that the potential shouting and general disruption might adversely affect the academic process. Before the oral exam, Bockris held a meeting in his office to discuss procedures, although one member of Packham's committee, Dr. M. Soriaga, did not attend. Bockris also asked that two people knowledgeable about tritium production in the cold be members of Packham's oral exam committee. These were famous electrochemists at the time in their own right. One was Dr. Norman Hackerman, President Emeritus of Rice University. Hackerman is a well-known electrochemist, who had also been to South Korea where he had seen the work being carried out there on tritium production in the cold. He had informed Bockris after that trip that he had seen tritium produced under completely different circumstances from those that Packham had employed.

The late Prof. Ernie Yeager of Case Western, the second participant requested by Bockris, was perhaps the most well-known physical electrochemist in the United States. He had been President of the Electrochemical Society, and had received many other honors. Bockris knew that he had obtained tritium in research with Robert Adzic, but that he had chosen not to publish the work, possibly because of the general atmosphere of ridicule of the topic. When they arrived at the lecture theater for the oral, it was full of people. Usually, these orals take place in small rooms, and the persons who attend are just the members of the committee concerned, about four people, and the candidate. Legally there can be other members of the university present, and it was already known that that this would likely occur.

The examiners, Hackerman, Yeager, and two professors in Biochemistry, together with Bockris, the Chairman of the Committee, sat in the front row. A large number of graduate students were there. Dean of Science John Fackler was there, as was Prof. Michael Hall, Head of the Chemistry Department at the time.

Packham began by summarizing his whole thesis. He talked first about his bacteriological work on the decomposition of water to yield hydrogen. He seemed to Bockris to be spending too much time on this—everyone present had come mainly to hear about tritium and not about bacteria decomposing water. Bockris asked him to get on with the tritium story.

Bockris as chairman of his doctoral committee had the task of choosing among the many hands held up at the time of questioning. Kevin Wolf was present, and Bockris favored him because he thought it would be most fair. Wolf had greatly opposed the work, and now was his time to say why. Wolf was allowed eight minutes to question Packham. After Wolf's questioning, Bockris exposed Packham to many other questions. After half an hour, Bockris was just about to close the discussion when Dr. Soriaga rose to his feet and walked down the aisle with a bunch of papers in his hand. He handed them to Packham and said: "Answer these!" Packham stared at the sheaf of papers, each of which contained a question. It was obviously impossible for him to deal with this publicly—it might have taken a couple of hours. One of the time constraints was that Bockris had to get Hackerman back to Houston in the ground transportation that had been arranged and was waiting.

Bockris queried the graduate school representative who was sitting in the front row, "What now?" He recommended that Packham should be asked to respond to these questions in writing, which could be included at the back of the thesis. Bockris announced the decision and the oral exam ended. The audience left and the committee remained, including Dr. Hall, who asked if he might be present at the subsequent deliberations. Bockris agreed.

All the members of the committee were quick to assent that they were satisfied with Packham's performance. They thought that his work certainly came up to the standard that deserved a Ph.D. degree. Only one person, Dr. Soriaga, held out against the work. He said that he could not sign the thesis because the formation of tritium in the cold was "impossible." Subsequently, Soriaga became rather heated. Dr. Michael Hall then made a suggestion: acceptance of the thesis should be conditional on replies to Soriaga's many questions being printed out in the thesis as an Appendix. Would Dr. Soriaga then sign? Yes, he agreed to that. It was the end of the oral examination and all the people present, except Dr. Hall who was not part of the Committee, signed the official forms which are generally regarded as giving the graduate student his Ph.D. degree.

They went up the stairs to the lecture theater and at the top of the stairs Dr. Hall shook Packham's hand, and said "Congratulations on your Ph.D!" Packham had, of course, been outside the lecture theater while the deliberations were conducted. He had become rather anxious, because usually these deliberations last ten minutes and this had lasted more than a half-hour. Hackerman left for his trip home and Bockris invited Fackler, Hall, and Yeager to come with him to the Plaza Club in Bryan for what turned out to be a pleasant dinner. The academic process had worked satisfactorily—an oral exam had been completed in a very controversial area.

The next day, however, everything had changed. One of the conditions, which is usually assumed to be a formality, still had to be fulfilled in the awarding of the Ph.D.: the department head's signature. Usually after the graduate school committee completes its recommendation, the papers are sent to the head of the department and he routinely signs off the thesis. Dr. Hall refused to sign off. His handshake with Packham and congratulations had evidently not been meant seriously! He now said that he couldn't accept the thesis either, because it was well-known that tritium could not be formed in the cold.

The next few days became a furor of negotiation and discussion. Finally an arrangement was worked out, largely on the suggestion of Kevin Wolf. Packham would rewrite his thesis, cutting out all reference to tritium production, and the thesis had to stand or fall on the basis of the biological work alone. Packham would be allowed to have an "appendix," which would consist not of the answers to Soriaga's questions, but the very papers he had already published in refereed journals on the formation of tritium. This arrangement was agreed to by the biochemistry professors, who said that the biochemical work that Packham had done was "just enough" for a Ph.D. degree. Now Hall signed the thesis and Packham had his degree. Today, Dr. Packham works for Lockheed on NASA's closed-cycle life-support systems for long-duration spaceflight, such as manned missions to Mars. He occasionally is seen on CNN or other television coverage of these exciting projects.

The matter was not quite over. A journalist for the *Dallas Morning News* had been in the audience for the oral exam. Her two-page article appeared in the paper's Sunday edition, describing the oral exam in detail. Bockris was pictured suppressing the discussion, while not allowing the junior members to speak properly or to ask questions. Nothing was said about the tension—the torture—applied to a student who had worked for six years on his Ph.D., had been congratulated on having done it by the Department Head, being certified as having it by the graduate committee, and then having it torn from him at the last moment by the Department Head's overnight change of mind.

The Department Head sent around a memorandum to the faculty the next day after Packham's oral. He promised that no other orals of this type would ever occur. He apologized to the junior professor, Dr. Soriaga, whose feelings he claimed had been hurt. He wrote, "You have witnessed the chairmanship of a committee by an autocratic professor. . ." Bockris sent this note to the Smithsonian

***Triumph continued on page 67***



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# Summary Report on ICCF8: The Eighth International Conference on Cold Fusion, May 21–26, 2000 Lerici, Italy

by Jed Rothwell and Eugene Mallove

*Photos by Barbara DelloRusso*

The Eighth International Conference on Cold Fusion (ICCF8) convened May 21-26, 2000, in Lerici, Italy at the antique and beautiful Villa Marigola Conference Center. The prior conference, ICCF7, was held in Vancouver, B.C., Canada, in April 1998. Following the traditional North America, Europe, and Asia rotation, ICCF9 will be held in Beijing, China, May 2001, with some anticipated official scientific sponsorship, according to ICCF9 organizer Prof. X.Z. Li

ICCF8 was unusual in having significant sponsorship support from multiple official Italian scientific organizations. The sponsors were: ENEA (Italian Agency for New Technologies, Energy, and the Environment); CNR (National Research Council); INFN (National Institute for Nuclear Physics); and SIF (the Italian Physical Society). The ICCF8 conference proceedings will be published by the Italian Physical Society in a peer-reviewed version in January 2001.

**Overall Impressions**

There were no great surprises at this conference. A few researchers reported steady, reliable progress. A few were still floundering around in the dark, making mistakes they should have learned to avoid years ago.

This conference may have exposed two long-established ideas as myths: 1) High loading is essential to all forms of cold fusion (see Storms and Warner presentations below); 2) Cold fusion experiments are inherently difficult to perform (see Warner). High loading may not be needed after all, at least not with Ni, Ti, and Pt. It may be necessary with Pd, but that is probably the "worst choice" of metals, as Storms put it.

The best news from ICCF8 is that spectacular progress has been made in correlating helium-4 and helium-3 with excess heat, especially at SRI International (McKubre, 29) and Osaka University (Isobe, 14). [Editor's Note: The number after the investigator's name indicates the number of the abstract on the official ICCF8 list.]

The disappointing news was turnout. As expected, only about 145 people showed up, the lowest number yet for an ICCF, although this was the first time a conference had been held in late May, when the end of academic year events may have interfered. Jed Rothwell's comment: "The number of participants is asymptotically dropping to zero because almost no young people participate. Most of the researchers are retired professors, 65 to 75 years old. An actuarial table will show that unless young people join the researchers, the final conference will take place in about ten years, after which most major participants will be incapacitated or dead! Most top researchers have already retired or died. You would think that scientists understand statistics and mortality rates, but they act as if they have all the time in the world."

Dr. Francesco Scaramuzzi

Co-compiler of this report, Eugene Mallove, is more sanguine on this matter of attendance: Certainly, most ICCFs have drawn on the order of 200 participants, with a high of about 350 at ICCF3 (Nagoya, Japan, October 1992). There were many who had indicated an intent to come to ICCF8, but for whatever reason did not make it—among these were those who are known to be very active in the field, such as Dr. Claytor of Los Alamos National Laboratory. The ICCF7 Proceedings were dedicated to the memory of M. Okamoto, and ICCF8 will be dedicated to the memory of G. Preparata, who died in April 2000 (see obituary, p. 8).

A list of ICCF8 authors is posted at: http://www.frascati.enea.it/ICCF8/programme.htm. Most of the listed authors showed up.

The conference organizers said they would make continued use of the web page to publish things, such as the list of participants. Several people at the conference called upon authors to make better use of the Internet. Much lip service has been paid to the advantages of the Internet in previous ICCF conferences, but little has been done so far. Ed Wall of our New Energy Research Laboratory (NERL) suggests that abstracts and author addresses should have been made available to participants *before* the conference, which would allow participants to study the abstracts in advance, prepare questions, and correspond with the speaker, "making it a much richer experience for all." On the other hand, many presenters and attendees may prefer the element of surprise or anticipation and would not wish for advance notice.

The conference organizers videotaped the presentations (as did we, informally for our research purposes, as did also our colleague Akira Kawasaki). The ICCF8 organizers will be selling their video tapes at a "very reasonable" cost, they said. They displayed the video input on two large projection-screen televisions at the front of the conference room, which helped those who could not see the viewgraphs. Their cameras and microphones were well-placed and professionally handled, so the video tapes should be of good quality.

We present a selection of conference highlights:

**F. Scaramuzzi, M. Fleischmann**: Lectures in memory of Giuliano Preparata, centering on his contributions to theory of coherent nuclear reactions. Scaramuzzi briefly described the Italian government initiative in cold fusion, and Fleischmann said the program may include work in other controversial areas such as the effect of magnetism on health, which, as he put it, "will make us even more enemies." From Jed Rothwell's perspective, this may have been a counterproductive thing to mention, given the already perilous state of cold fusion research. Mallove's view: If theory designed to address anomalies of coherent atomic-nuclear energy release in water-based systems has other implications, such as in medicine, cold fusion scientists should not feel constrained to address these simply out of fear of giving cold fusion opponents more targets. (It is noteworthy that as of mid-June 2000, perennial critic Robert Park of the American Physical Society had not yet mentioned ICCF8 in his "What's New" column—either a sign of emerging cold fusion ascendancy, or terminal boredom with the topic by Park.)

**A. De Ninno *et al.* (93):** "The Fleischmann-Pons Effect in a Novel Electrolytic Configuration." Thin wire Pd deposited on glass. This is a miniature, or even microscopic, version of the electromigration in wire experiments which have been popular in Italy for several years, work which Dr. Celani and his group has done. These experiments are based on the theories of Fleischmann and Preparata. Some Americans, including Storms, do

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not think that electromigration can produce as much pressure within the metal lattice as the Italians claim. These Italians may be putting too many eggs in one basket by concentrating so much on this approach. This presentation needs to be examined more carefully, but on the face of it, the calorimetry is unconvincing, because the devices are so small and power levels so low: around 50 mW total (input plus apparent excess heat). The levels of material purity claimed are also suspiciously high. The wires are so fine, they frequently de-laminate from the substrate. We do not see the point of making such tiny devices, but Peter Hagelstein of MIT pointed out they have the advantage of loading quickly and starting the cold fusion reaction quickly. Perhaps, but as far as we know, the thin film Pd devices do not turn on any faster than the Ni or Ti cathodes, which have thousands of times more mass, and which make stronger, more easily measured excess heat.

**P. Hagelstein (64):** "A Model for Fast Ion Emission in Metal Deuterides." A continuing and now much more robust theory paper than Hagelstein of MIT has ever delivered—seeming to approach the stage where calculations of effects from system descriptions and reaction products might be carried out. Hagelstein told us he has been working more closely with McKubre at SRI International recently, and they have successfully designed experiments which address theoretical questions. McKubre's presentation confirmed this close collaboration. Hagelstein sees significant progress both with theories (his own and the Chubbs' in particular) and he thinks there is better agreement between data and theory now than in previous ICCFs. He seemed quite optimistic.

Dr. Peter Hagelstein

**F. Celani *et al.* (96):** He weighed in with the longest title we may have seen: "High Hydrogen Loading of Thin Pd Wires Through Alkaline-earth Carbonate Precipitation on the Cathodic Surface: Evidence for New Phases in the Pd-H System. Unexpected Problematics Due to Bacteria Contamination in Heavy Water." In the final portion of the lecture, Celani explained that they have discovered bacteria which survive remarkably well in heavy water, and which may be a previously unknown species. This is interesting, but a distraction. Eugene Mallove, who sat near critic Douglas Morrison of CERN, observed that this discovery by the Celani group seemed to amuse him, perhaps to provide grist for future Morrison commentary.

**Y. Iwamura *et al.* (59):** "Nuclear Products and Their Time Dependence Induced by Continuous Diffusion of Deuterium Through Multi-layer Palladium Containing Low Work Function Material." Iwamura's already formidable experiment has been improved. His group now performs continuous *in situ* spectroscopic analysis of apparent transmutations in a multi-layer thin film metal sample. The apparatus is installed in a clean room and the cell is sealed throughout the experiment. Deuterium gas is diffused through the diaphragm "cathode" by producing a vacuum on one side of it. (It is not strictly a cathode, or an electrode, but that is what they call it.) The sample is not removed from the apparatus during the experiment, making contamination unlikely. Analysis *in situ* is performed with XPS (X-ray Photoelectric Spectroscopy) and SIMS (Secondary Ion Mass Spectrometry), and after the experiment with ICP-MS (Inductively Coupled Plasma Mass Spectrometry). With the Pd-D sample, Mg, Si, and S appear in the Pd, and increase monotonically as the experiment progresses. The Si and S isotopic abundance is not natural. X-rays are observed. A Pd-H blank sample run in parallel shows no excess heat, significant element formation, or X-rays.

Iwamura works for Mitsubishi Heavy Industries, in its Advanced Technology Research Center. Several large Japanese companies are continuing to support research projects (despite the demise of the official program, see *IE*, No. 30, p. 26). Some corporate projects are surreptitious. They sent representatives to the conference, but they do not want to go on record and we would not want to endanger the funding by naming them. They might even be collaborating with US-based companies. The researchers are grateful for the funding, and express their appreciation to their anonymous corporate benefactors, and so do we. Occasionally the behavior of these corporations is dumbfounding. After Toyota made a show of abandoning cold fusion research a few years ago, two other Japanese automobile companies jumped in. They explained to a startled Japanese researcher, "now the field is wide open"—as if only one company can do research at a time.

**J.J. Dufour *et al.* (40):** "Hydrex Catalyzed Transmutation of Uranium into Lead." Uranium hydrides were subjected to magnetic fields and electric currents. This seemed to increase the speed at which uranium converts to lead by a small but easily measured extent, and it produces commensurate excess heat. The samples were 500 to 900 mg, and they initially contain 1.5 to 2 ppm of lead, evenly deposited throughout the sample. After the treatment, the "periphery" of samples (presumably meaning the surface layers) contain 2000 ppm of lead, or 20 ppm for the entire sample. Gamma activity decreases by up to 1%. The lead is measured by dissolving the sample and measuring with ICP-MS. Dufour believes that cold fusion is caused by what he calls "HYDREX" (electromagnetic metastable proton electron resonance), and like most experimentalists with a pet theory, he always seems to find evidence for the theory. However, people with other theories were impressed by the care with which these experiments were performed, and some of them found support for their own ideas. There have been other reports of radioactive decay being sped up or enhanced. The first and most impressive example was in a series of experiments performed in the 1960s by O. Reifenschweiler, which were later reported in *Physics Letters A*. Also, there are a host of groups such as CETI, Cincinnati Group, Trenergy, and Monti America, which have or continue to claim success with low-energy remediation of ingredients of nuclear waste.

**J. Warner and J. Dash (6):** "Effect of Cold Work on the Amount of Excess Heat Produced During the Electrolysis of Heavy Water with Titanium Cathodes." Warner is a graduate student with Prof. John Dash at Portland State University (Oregon). He gave a fine lecture. These results were described at the recent APS meeting in Minneapolis in March 2000. The group continues to make progress. This experiment is relatively foolproof and it nearly always produces excess heat with high confidence. One of the reasons it works so well is because they stick to the "KISS" formula—"Keep It Simple, Stupid!"—meaning they do not burden the experiment with complex instrumentation, or attempt to measure anything other than excess heat *in situ*. After the run they look for evidence of anomalies and transmutations with electron microscopy and EDX at Portland State, and with NAA at another lab, Reed College. This simplicity is an advantage and a disadvantage. Storms pointed out that this method cannot reveal much about the reaction. You cannot

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measure electrochemical conditions and control parameters such as OCV (Open Circuit Voltage), or look for helium or X-rays with such simple equipment. When you add in on-line detectors, the experiment becomes complicated and difficult. Still, it would be nice if other researchers would master the art of generating excess heat and doing reliable calorimetry before they launch ambitious programs to measure helium or X-rays. Another problem with this experiment is that the acidic electrolyte erodes about 40% of the cathode, destroying most evidence of transmutation or interesting surface changes. On the other hand, this bolsters his contention that some spots on the surface metal are transmuted, because it is unlikely that contamination would stick to the metal.

Warner does not think that loading is an issue with this experiment, because the effect either turns on immediately or not at all, and the material does not appear to form a titanium deuteride.

Jon Warner

Our group at Cold Fusion Technology, Inc. (New Energy Research Laboratory, NERL) will soon attempt to replicate these titanium excess heat results with an identical Calvet or SEC (Seebeck Envelope) calorimeter (made by Thermonetics, Inc.), and Storms will also try to do this when he gets a chance. He has done a preliminary test of a sample provided by Warner, which showed no excess heat. Dash and Warner have been quite helpful. If other groups can replicate Warner *et al.*, it will bolster the claims. Warner says he would be delighted, particularly if NERL or Storms can do it before his oral doctorate examinations. Compare Warner's response to that of another company (our good friend CETI), which threatened to sue anyone who attempted to replicate their bead cathodes. Multiple replications would prove that cold fusion is easier than most experts have claimed over the years, and the missing element has been good teaching, enthusiasm, and the will to show other people how to do the work. Every summer for several years Dash has hired two or three high school students to perform cold fusion experiments. Every year they fail at first, for prosaic reasons such as leaky seals, contamination, or bad electrical connections. After a few failed attempts, the students learn how to produce excess heat. Dash's calorimetry has improved over the years. We very much doubt he is making a mistake, although one cannot be sure until he is replicated. Dash and Warner now use an advanced Calvet calorimeter along with an array of eight isoperibolic cells wired in series. They ignore an excess heat signal under 100 milliwatts, that is, only two or three sigma. This is a conservative estimate of the error margin with the Calvet calorimeter. Other cold fusion scientists should be as careful, summarily rejecting low-sigma results. It would give people more confidence in this field.

Dr. K.P. Sinha

**M. McKubre *et al.* (29):** "The Emergence of a Coherent Explanation for Anomalies Observed in D/Pd and H/Pd Systems." There is so much to say about this, it is hard to know what to squeeze into this preliminary note. McKubre has outdone himself with the Case and Arata replications, which are far better than the originals. The helium issue is now closed. Cold fusion produces helium *commensurate with heat*, meaning the helium-4 production rate is comparable to a hot fusion deuterium-deuterium (D-D) reaction yielding helium-4 plus a 23.8 MeV gamma ray, only there is no gamma ray, just equivalent energy in the form of heat. The helium can only be a product of the reaction, not contamination, for several reasons: mainly because in 4 out of 16 cases it was measured at levels far above atmospheric concentration. (In other words, the helium may have leaked out of the cell, but under no circumstances could it have leaked in.)

The helium-3 and tritium results were also definitive. The ratio of helium-3 to helium-4 in nature is about 1:800,000. With the Case replication it reaches 1:67, 12,000 times higher than in nature, and in the inner chamber of the Arata replication the ratio is 44,000 times higher than nature. McKubre thinks the helium-3 is mainly the product of tritium decay, so we may now soon add McKubre's group to the list of those generating tritium in cold fusion experiments.

Huge amounts of tritium were found in some cells. The researchers devised a machine to pierce the Arata double structured cathodes and extract a sample of the gas inside them while rigorously excluding outside contamination. Samples were tested at one of the world's most sensitive mass spectrometers, in a laboratory specializing in the detection of helium, hydrogen, and other light elements at McMaster University. One sample of unknown contents from a newly opened cell turned out to have over 1000 times more tritium than anything previously submitted to that instrument. It put the instrument out of commission for three months!

Dr. Michael McKubre

Here is a quote from McKubre's summary at the end of his talk: "These results obtained in three different metal sealed cells, by three different calorimetric methods, with both electrochemical and gas loading experiments. The helium-4 analysis have now been performed in four different institutions, helium-3 analysis in two different institutions."

Helium has been detected for many years by different researchers, but never in such large amounts, at such high concentration. It has never been observed with such large signal-to-noise ratios, or produced so consistently and reliably. The Case cell works about two-thirds of the time at SRI International, and the Arata DS-cathodes work *every* time. These are difficult experiments and it is extremely unlikely they would work as smoothly in laboratories with less skilled researchers. This is no claim of "easy replicability," such as Warner and Dash make.

After the lecture, McKubre was asked about evidence of transmutations or other changes to the cathode material. He said it was too early to comment on this subject. Tom Passell, retired from EPRI, told us that he has acquired some of the used cathode material and he is conducting the analysis. He has subsidized, below-cost access to high resolution mass spectroscopy.

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In this research, SRI cooperated closely with Osaka University, McMaster University, Pacific Northwest Labs, ENEA, and individuals including Les Case, Russ George, and Tom Passell. This openness and wide-ranging collaboration is exactly what this field needs most. SRI, McKubre, Tanzella, and Tripodi should be congratulated for these spectacular results.

**J. Kasagi *et al.* (71):** "Low Energy Nuclear Reactions in Solids." Continuing work by the group from Tohoku University in Japan, which demonstrates that with very low-energy bombardment of metals, such as palladium, by deuterons and lithium ions, unexpected enhancements in the reaction rates of D + D or Li + D occur. The implications for cold fusion are many.

**E. Storms (32):** This was originally slated to be about thin layers of Pd on inert substrates, but Storms has not had much luck with those cathodes, so instead he spoke about excess heat from Pt—yes, that's right, platinum! These results are presented in detail at:

http://jedrothwell.home.mindspring.com/Pt-energy.htm

http://jedrothwell.home.mindspring.com/Active-Pt-quattro-pro.htm

Furthermore, Eugene Mallove of NERL visited the Storms laboratory in April 2000, and found a cell active at that time producing excess heat from a Pt-Pt cathode-anode combination. The data and care with which they were obtained were extremely convincing. Note well, ye skeptics: This does *not* mean that Pt-Pt control-experiment methodology has been overturned—read on!

Storms now has two active Pt cathodes. He had some interesting new things to say about this experiment. During the lecture, Storms pointed out that this is the first cold fusion experiment in history to be fully published on the web, including all data points (in spreadsheets).

Dr. Edmund Storms

When Storms cleaned off the Pt and removed all depositions from the surface, especially by placing the cathodes in boiling water for a while, the cathodes ceased producing excess heat. He then took steps *to deliberately build up layers of deposition*, by putting the cathode in concentrated electrolyte, that is, high molarity electrolytes, made by dissolving a lot of powder reagent in water.

Michael Melich told Jed Rothwell, "This is old news. Pons and Fleischmann reported excess heat from Pt in 1989." It may be old news, but no one else has followed up on it actively to our knowledge. Excess heat from Pt has been the guilty secret of cold fusion, perhaps because it seems to show that any metal will produce heat under any conditions, and there are no "blank" (null, or control) experiments. The instruments always show heat sooner or later, with any combination of heavy water, light water, Pd, Pt, Ti, Al, or even Au or W. The skeptics long ago seized on this and said it proves that *all* excess heat must be an instrument artifact. They may have a partial point, albeit not for the right reasons. We suspect that many reports of heat from oddball configurations with non-standard calorimeters may be, in fact, artifacts.

Storms' conclusions:

• Up to 800 mW of power, and 330 kJ of total energy was produced by the Pt sample, in completely reproducible patterns of behavior.

• There is a linear relationship between applied current and excess power.

• These results suggest that other metals which produce this effect may also involve surface layers (rather than the bulk).

• Many researchers . . .who are committed to understanding palladium, should have an open mind, and consider that perhaps they are barking up the wrong tree.

• It isn't really the metal producing the excess heat effect; it is something being deposited on the metal, and palladium is not the best substrate, because it takes time for the palladium to load such that it can support the surface layer of "X."

Storms concluded by saying, "Indeed, people ask me what am I using to calibrate; what is dead . . .?" [In other words, if Pt produces heat, what metal never produces heat and can be used as a reliable control blank.] "Clean Pt is dead—*there is no doubt about that*. Obviously it can come alive. . . With some humor and irony, I suggest you use palladium [as a control]."

**X.Z. Li (62):** "Nuclear Physics for Nuclear Fusion." Theoretical discussion of a resonant mechanism whereby a large cross-section for fusion without strong neutron or gamma emission may be obtained. A theory from which *both* cold fusion and hot fusion can learn.

**G.G. Miley (65):** "Advances in Thin Film Electrode Experiments." At first glance this looks impressive, but we have very strong doubts about the calorimetry and think thin film on glass is a poor choice of materials. The thin film pulls off too easily (de-laminates) and it does not have as much surface area as beads or convoluted structures like coated Ni fibrex, which looks like compressed steel wool.

Dr. George Miley

We at NERL helped arrange the funding for this research and frankly, we are very disappointed by the results so far, although the thin Pd-wire experiment that showed a heat burst at a high loading ratio is intriguing (but not definitive and not reproduced, as yet). Miley and his students did good work a few years ago working with CETI, but unfortunately that partnership failed, the University of Illinois was upset with CETI, and the graduate students who were working on that project graduated. The new crop of people made major errors. Apparently, Miley was so busy with his other commitments, including hot fusion projects and meetings, that he did not adequately supervise the project, so it went badly off the track, although he thinks it has gone well. Storms also has doubts about the Miley

Dr. X.Z. Li and Prof. Akito Takahashi



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calorimetry and he is preparing to test Miley thin film cathodes when they can be delivered to him. The Storms and NERL tests of the Miley cathodes will determine whether such cathodes can be used for demonstration cells, as we had hoped.

Dr. Ubaldo Mastromatteo

**U. Mastromatteo (42):** "An Energy Amplifier Device." At first glance we find this unconvincing, but intriguing. The author, a senior researcher with an Italian solid-state electronics firm, described a thin film device manufactured like a semiconductor. (We will present the author's full paper in a subsequent issue of *Infinite Energy*.) The only major indication of cold fusion is apparent excess heat, and the calorimetry is based on secondary or tertiary evidence from the metal melting point and loading behavior, even though the device is small and low powered, and could easily be installed in a conventional calorimeter. This research is well-funded, so there is no reason for such inadequate instrumentation. It is possible that the apparent excess heat may be caused by stored energy in the melted substrate (a phase change which does not show up as a temperature increase).

Mastromatteo is at SGS-Thompson Microelectronics, one of Europe's largest semiconductor companies. (http://us.st.com/stonline/index.shtml). The company has vast experience fabricating similar devices, deep pockets, and they are seriously interested in developing a commercial heat source. That's wonderful, but why haven't they done adequate calorimetry yet, since the work was initially reported at ICCF7 in 1998? One possibility: Dr. Mastromatteo told Eugene Mallove that this project does not have high priority within the company's lucrative traditional business.

**Y. Arata and Y.C. Zhang (18):** "Definitive Difference among [Bulk-$D_2O$], [DS-$D_2O$], and [DS-$H_2O$] Cells in the Deuterization and Deuterium-reactions." This was most certainly a very important paper, and it may have been definitive, but alas we have few clues about what Arata said. His English is so poor and halting, that despite thirty years of experience listening to Japanese-accented English, Jed Rothwell could not make out what he said. His viewgraphs do not help much either. They show a tangle of graphs too small to read from a distance, and tables without headings. We will review the video tape, but will probably not understand what he has in mind until the proceedings are published in January 2001. Note, however, that McKubre at SRI has obtained remarkable confirmatory excess heat and helium results with Arata's double-structured cathode.

Dr. Yoshiaki Arata

**T. Passell and R. George (100):** "Impurity Analysis of Palladium Exposed to $D_2$ and $H_2$." Passell performed neutron activation analysis on four samples of Arata's double cathode powders which had been exposed to many atmospheres of pressure of either deuterium or ordinary hydrogen. Four samples appeared to show evidence of enhanced zinc isotope ratio content—factors ranging from 6 to 14 of exposed samples relative to virgin samples. Tests on samples obtained from Dr. Li and colleagues at Tsinghua University showed similar results for ordinary hydrogen exposure. This is a fascinating paper about analytical work that was financed on a shoestring by Passell.

**M.H. Miles (58):** "Calorimetric Studies of Palladium Alloy Cathodes Using Fleischmann-Pons Dewar Type Cells." A description of the research at the NHE laboratory, similar to the version that was published in *IE* No. 30.

**Y.E. Kim and A.L. Zubarev (33):** "Ultra Low-Energy Nuclear Fusion for Bose Nuclei Confined in Ion Traps." A theory paper by this top theorist in nuclear physics from Purdue University, who has been in the cold fusion field from the outset.

**V. Violante *et al.* (34):** "Hydrogen Isotopes Interaction Dynamics in Palladium Lattice." A complex study performed in cooperation with SRI International.

**T. Mizuno *et al.* (103):** "Production of Heat During Plasma Electrolysis in Liquid." The tungsten glow discharge experiments described in many articles in *Infinite Energy*, beginning with Issue No. 20, and now discussed here. Mizuno showed three new color photographs of the plasma that forms over the cathode. The form and color of the plasma changes as excess heat develops. We hope to publish a copy of the photos on a web page soon.

Dr. Vittorio Violante

An interesting positive replication of this experiment was reported informally by Peter Mobberley from the UK (see also in this issue, Testing Update, p. 38). He uses a "long reach" tungsten spark plug as the cathode. The outside electrode is cut off and a large stainless steel iron bar is used as an anode. That is an excellent choice of materials, because spark plugs are standardized; they are made of rugged, well-tested alloys; and the ceramic packaging is ideal for this purpose. Mobberley reports ~70% excess heat, highly repeatable. He has crude but seemingly adequate instruments: a

Dr. Tadahiko Mizuno

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Variac and an analog three-phase electric meter. Since he is in the UK, the mains voltage is high, which is convenient for this experiment, which begins to work well at 150 to 200 volts when electrolyte temperature is high enough, about 80°C. Mobberley also discussed with us privately a variation of the experiment with a plasma formed in a ceramic separator placed between two electrodes, in highly concentrated electrolyte. We at NERL will attempt to replicate this soon.

An interesting connection: Storms discovered that the excess heat effect does not take place in the Pt bulk, but rather in layers of deposited material on the surface of the Pt. When he cleaned this Pt surface off by scrubbing the metal and placing it in boiling water, the effect went away for a while until another layer of deposited material grew on the metal surface. Mobberley takes this a step further, moving the reaction site away from the metal to holes in a ceramic—in fact, he employs a pepper-shaker (known as a "pepper-pot" in the UK). Rothwell's first reaction was to say that this removes metal from the picture altogether and makes the effect look like something that happens in water, but Mallove reminded him that there is a great deal of lithium in the highly concentrated electrolyte used by Mobberley.

Mobberley's technique, if successfully replicated, might overthrow some accepted cold fusion "dogma." Perhaps the Ohmori and Mizuno glow discharge at the metal surface, and the Mobberley discharge in the middle of the electrolyte, far removed from the metal surface, is a separate phenomenon that has nothing to do with that standard approach to cold fusion. It is Mobberley's opinion that it may have to do with much-discussed lithium-proton or lithium-deuteron reactions, leading to helium-4 formation. Wouldn't that be lovely!

Dr. Alexei Roussetskii

**M. Swartz (70):** "Metanalysis of Patterns of Success in LENR/CF." Mitch Swartz did not attend the conference, but his analytical paper was published in our last issue, which was distributed at ICCF8.

**A. Roussetskii (75):** "Application of Cr-39 Plastic Track Detector for Detection of DD and DT Reaction Products in Cold Fusion Experiments." Numerous charged particle tracks are seen from deuteron bombardment at low energy of titanium deuteride. Evidence of $^3$He-, triton-, and proton-generated tracks.

Dr. Scott Chubb and Dr. Edmund Storms

Dr. Talbott Chubb

**E. Del Giudice (92):** "A Simple Model of the 'Cohn-Aharonov Effect in a Peculiar Electrolytic Configuration." This should be spelled "Coehn-Ahronov," meaning the electromigration effect, of which Martin Fleischmann is so enamored. Since Giuliano Preparata had put considerable theoretical effort into the effect, it is now to be called the "Preparata Effect."

**Y. Isobe *et al.* (14):** "Search for Coherent Deuteron Fusion by Beam and Electrolysis Experiments." A very impressive set of three experiments conducted at Osaka University. In experiment 1, with conventional bulk palladium electrolysis, low levels of excess heat and up to $10^{16}$ atoms of helium were detected. In experiment 2, a 3 KeV electron beam struck palladium and titanium deuterides targets, and charged particles and X-rays were detected. In experiment 3, highly loaded titanium deuterides was irradiated with a deuterium or proton beam. The lecture was not long enough to do justice to three experiments, but details were available during the poster sessions.

**G. Mengoli *et al.* (7):** "Anomalous Effects Induced by $D_2O$ Electrolysis at Titanium." Titanium was electrolyzed in $D_2O$ 0.6M $K_2CO_3$ at a high temperature: 95°C, in an externally heated cell. The calorimeter incorporates a Dewar cell and an external condenser. We have not looked at this closely, but water-based calorimetry at these temperatures is extremely difficult, because heat lost as water vapor is difficult to measure accurately. This research is well-funded, and we do not understand why they do not employ something like a Calvet calorimeter. The authors claim the initial heat output was ~200 mW, and it later increased to ~2 W. They also claimed there were hours of heat-after-death when electrolysis power was turned off and external heating continued. This may be difficult to believe, because it would be unprecedented for titanium electrolysis. The calorimetry looks questionable, but Mengoli also showed surprisingly strong evidence for transmutation of titanium into a radioactive scandium isotope, with what looked like unassailable evidence: gamma ray coincidence counting and determination that the half-life of the gamma decay was consistent with the radioactive isotope as identified by the energy of the gamma ray spectrum. (In many experiments calorimetry is more believable than spectroscopy, but not always.)

**S. Chubb and T. Chubb (25):** "Theoretical Framework for Anomalous Heat and Helium-4 in Transition Metal Systems." An enthusiastic and clear performance, as always. Chubb compared some aspects of cold fusion to HTSC, the Mössbauer effect, and other well-established phenomena; he ended by suggesting that in these effects, groups of atoms band together in what might be called a meta-particle (Jed Rothwell's term), and a change which affects them all simultaneously, which explains how the heat manages to couple to the lattice at the speed of a nuclear reaction, without tearing apart the host metal. He thinks this is true simultaneity, or as one member of the audience called it, "superluminal electrolysis."

**H. Kozima (46):** "TNCF Model—A Phenomenological Approach." A highly unconventional theory, which postulates the existence of "quasi-stable thermal neutrons" or free neutrons floating around inside the lattice for indefinitely long periods of time. Several theorists suggested privately that this

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should have been relegated to a poster session, but Dr. Kozima has been most insistent on the validity of his theory. He has tried to connect it with other theories with which he finds kinship. Battles over cold fusion theories continue to generate much "excess heat" within the field.

**M. Fleischmann *et al.* (106):** "'Case Studies of Experiments Carried out with the ICARUS Systems." A closer look at some of the apparent mistakes made in the Japanese NHE (cold fusion) program. In *IE* No. 30, p. 31, we listed some of the reasons why Fleischmann disputes the NHE's negative conclusions. Unfortunately, Fleischmann was pressed for time, and he reiterated these and other reasons in such detail and so rapidly that we expect that few people in the audience were able to follow the discussion. (His talk prompted the only halfway impolite, argumentative question from CERN's Douglas Morrison. At other ICCFs Morrison has repetitively challenged presenters over what he regarded as inadequate testing protocols and theories. See page 32 for more on Morrison.)

Dr. Martin Fleischmann

### The Loading Issue

Since 1989, it has been taken for granted by most researchers that the cold fusion effect occurs in the metal lattice, and that it requires high loading, that is, nearly as many deuterons in the lattice as Pd atoms. Fleischmann has long maintained that the reaction occurs in the "bulk," (inside the metal lattice) and not on the surface. In 1989, when Preparata began developing theories, he and others set forth two propositions which have been widely accepted as givens:

1) High loading is essential; the reaction takes place in the lattice. 2) The reaction does not generally produce neutrons or tritium; it produces mainly helium-4.

The second proposition has now been proved by McKubre's results, but the first is in trouble, and its unquestioned acceptance may have delayed the development of the field for many years. There is no question it is true in a sense, at least for Pd. It has been confirmed experimentally with as much confidence as helium-4 production. The questions now raised are: Why is it true; what is true about it; and can the benefits of loading be achieved by some other method more easily?

Yet Storms ended his talk by saying, "The idea that high loading is not necessary would be a stunning reversal, a sign that much research over the last ten years has been misguided. Few of the conference attendees are interested in hearing this news." There is no doubt high loading is necessary for excess heat in Pd. Many people have observed this, and McKubre's famous curve of loading versus heat proves that loading correlates closely with heat in Pd. However, evidence has now emerged that loading is not important with other metals, and with Pd it might only be needed in order to "crowd" deuterons at the surface. In other words, the deuterons crowded inside the bulk of the metal may contribute little or nothing to the reaction. Nothing happens until most of the bulk is filled with deuterons and they begin spilling out onto the surface, and only (or mostly) the ones at surface participate.

Dr. Melvin Miles

You might achieve the same results by eliminating most of the bulk; that is, by making the Pd a micron-scale thin film, or by using a metal like Ni, which absorbs few deuterons (or protons). As Storms put it, describing his own positive Pt results, there is "no diffusion, no loss rate; the ions stay at the surface." This is a naive, simplified model, but if anything like it turns out to be true it will be ironic that so much effort and so many millions of dollars have been spent to increase loading, if it is determined that loading plays no fundamental role in the reaction.

### Scaling Up, and Down

Many Italian mainstream researchers are working with ever-smaller devices, which are now on the microscopic scale.

**A. De Ninno (93):** She described a thin film "wire" 2 microns thick, 50 microns wide, and 80 centimeters long. They are based on the electromigration theories of Fleischmann and Preparata. The Italians may be putting too much emphasis on these theories, which may not be widely respected outside their circle. They may be betting too much research on them. They report several effects which they think are interesting anomalies, but which other researchers suspect may be prosaic instrument errors caused by the difficulty of working with such small devices. Problems such as tiny levels of impurities or a segment of the thin film that has torn off the substrate (de-laminated) might be extremely difficult to observe directly, and they might cause some of these "interesting" effects. For example, De Ninno reported an apparent cutoff of the Baranovski curve, which relates electrical resistance ratios to loading. She said the cut-off is predicted by theory, and after you take this cut-off into account, loading is actually extremely high. Other researchers say the curve may not really be cut-off, and loading is not so high. Because the electrolyte conducts almost as well as the thin film "wire," De Ninno must interrupt electrolysis power for a fraction of a second to measure resistance, and other researchers suspect that during that brief pause the wire is deloading, and losing a substantial amount of the deuterium in the wire.

Dr. Ivan Chernov
Poster session participant.

### Conference Summary

Friday morning, the last day of the conference, was a time to take stock of what had been accomplished at ICCF8. Conference organizer Francesco Scaramuzzi of INFN, Frascati noted that there had been twenty-six oral presentations and about fifty confirmed poster presentations. He remarked that of the 145 registered participants, the four leading countries were: Italy (41), U.S. (38), Japan (22), and Russia (12). He spoke of a "lively conference, rich with results." He said there had been "strong confirmation that we have the production of excess heat and helium and their correlation. Yes, there is excess heat and yes, it is of nuclear origin." He considered the work on transmutation presented at the conference to be "high level" and an "indication of the nuclear character of the phenomenon." In his opinion, cold fusion could only be understood in terms of coherence—an important aspect

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of the behavior within condensed matter, which he said would have more general implications for physics.

Scaramuzzi acknowledged that most of the scientific community does not think that cold fusion exists and that "it is not science." He called that an "incomprehensible position" in view of the peer-reviewed articles on the subject that have already appeared. Even so, he said that he considered cold fusion to be still in the stage of a scientific problem, not quite ready for the commercial arena. Differing with this position was Dr. Biberian from France, who said in his summary, " We are *sure* that it is real. . .We need applications to make the world realize it. . .We need to be more pro-active." He suggested that cold fusion people should not be like the fabled Cinderella, "waiting for her prince to come." Dr. Yury N. Bazhutov of Russia, also touching on emerging commercial interest, noted that more than half of the Russian participants had received financial backing from private companies to come to ICCF8.

In his overview, Prof. Akito Takahashi of Japan agreed that "science only is not enough." He said there was a need to extend interest in cold fusion to "industry people, people from other fields, and young people." He called the $^4$He-excess heat correlation "almost confirmed" and low-energy nuclear transmutation "nearly confirmed."

During the open-microphone session that followed the official summaries, an Italian researcher, Dr. Roberto Andreani, who heads ENEA's laboratory for *hot* fusion and is trying to get support for the ITER (thermonuclear) reactor, had kind words for the cold fusion community. He joked that he does not "receive a very good reception" when he talks about cold fusion to his hot fusion colleagues. He considers that the cold fusion work at ENEA is "very well done." Andreani urged that progress be made to create "absolutely convincing experiments . . .I wish you good success in your work, but remember that you have strong opposition."

Cold fusion researcher Dr. Jean-Paul Biberian made a special announcement near the end of the proceedings: the first international conference on *biological* transmutation had been held in Geneva, May 1, 2000. There had been sixteen attendees from Italy, Switzerland, and France. He had been doing experiments with bacteria and seed-sprouting already, observing unexpected element changes on the order of a few percent. A non-profit organization is being formed to support continuing conferences, publications, and research.

Dr. X.Z. Li of China, who is both a cold fusion and hot fusion researcher, had the last word. He is the organizer for ICCF9 in Beijing in 2002. "What will be the theme for ICCF9?" he asked. Answering his own question, "Coherence—in solid materials, in research *and* development, and in cold and hot fusion!" He looked forward to the day when cold fusion and hot fusion would "merge," since he believes that both communities are "working for the same goal: nuclear fusion without strong radiation." Well, at least *some* hot fusion people—those in *aneutronic* hot fusion—have that vision. Li suggested that cold fusion researchers should consider the possibility of developing a heater based on the "heat-after-death" phenomenon.

Dr. X.Z. Li

## Encounter with Dr. Douglas Morrison

Editor's Note: Dr. Douglas Morrison, a genial physicist now retired from CERN, the Geneva-based European high-energy particle physics facility, has been a perennial critic of cold fusion research. He has attended all the International Cold Fusion Conferences (ICCFs) since 1990, and has raised questions that have long since been answered to the satisfaction of every scientist working on cold fusion experiments. Yet Dr. Morrison continues to maintain that cold fusion research is "pathological science," a conclusion which he admits he reached in the earliest days of the controversy. Since his views are widely circulated, we thought it would be useful to document one particular set of exchanges at ICCF8 that involve him. —*EFM*

### Comments by Ed Wall, NERL

While walking to a session of the ICCF conference, I acquired the company of Douglas Morrison, who began telling me his opinion of the new book, *Excess Heat*, by Charles Beaudette. He was comparing it to Mallove's *Fire From Ice*, saying that Mallove's book was much less biased. I said that Mallove wrote the book fairly early on, and was giving consideration to both sides of the controversy, though he did conclude even in 1991 that the evidence for cold fusion was overwhelming. Morrison then started in on the Fleischmann faulty neutron measurement, hammering on it like it now still meant something. I emphasized that it was not significant because they were not physicists, so such measurements should not have raised such a fuss. After all, they admitted they made a mistake. No, Morrison was intent on impressing me with his rightness then and his rightness now. I didn't say much as I realized how sad the fellow is, but I did ask if there was anything at the conference he found interesting. He said, "There's nothing new." I asked about McKubre's presentation that showed his data for $^3$He concentration data four orders of magnitude above background in a Case cell. All he said was, "It's interesting." The next day, after Fleischmann's talk, Morrison informed Fleischmann and the rest of us infidels that he was there to help us do good experiments. I'm sure he had himself convinced at least.

Dr. Douglas Morrison

#### The transcribed exchange between Morrison and Fleischmann:

**M:** I sometimes feel my greatest contribution to [this field] is to say, "Do good experiments." Ed Storms said the technique of using the pulse of heat is not a good way to calibrate, you said Ed was wrong. Are you going to answer that? Now. . .

**F:** I haven't time.

**M:** Fine. The point is, you disagreed. Now when I say, "Do good experiments," I think one should do an experiment where you don't have these discussions. And the best way to do calorimetry, is to use the Wheatstone bridge method with a null. . .Where you try to get nothing, then you are safe. This means you have a calorimeter with three baths, at 30°, 40°, 50°. And you put the thing which you believe is putting out heat into the middle one, and all the others you keep constant. . .You reduce current going into the heaters [in other baths] to keep the balance. There is no argument about delicate calibrations. . . You avoid all these discussions. I would very much like to see people do good experiments and avoid all these discussions.

**F:** Well, you see Douglas, it is impossible to answer you. Because it would take about a half-hour. Your system is actually not applicable to the system we studied. You have to be very, very careful not to say you should use an isothermal calorimeter, like the plutonium-type calorimetry, which is not applicable to this type of system. You have to ask yourself, "What am I trying to measure?". . .I absolutely agree with you, that you have do it properly. Now in fact, the Harwell plutonium-type calorimetry [when applied to CF] was a complete fiasco. I could take about an hour to tell you why.

### Notes and Comments by Jed Rothwell

Morrison is confused about thermal pulses. Storms was talking about calibrating with a joule heater alone, without electrol-

*ICCF8 continued on page 68*



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# Phenomenon in Venezuela: A Documented Case of Unexplained Radiation Exposure

**Gordon David Kaswell***

An intriguing report made its way around the internet a few years ago, describing what appeared to be the case of a Venezuelan family's severe and unexplained radiation exposure. What was most remarkable about the account was the date of the event: October 24, 1886. Roentgen discovered X-rays in November of 1895, and Becquerel discovered radioactivity in February of 1896—roughly *nine years after the Venezuelan incident.*[1] Therefore, if the report proved accurate, a genuine anomaly would be present. I thought it was worthy of further investigation, and proceeded accordingly.

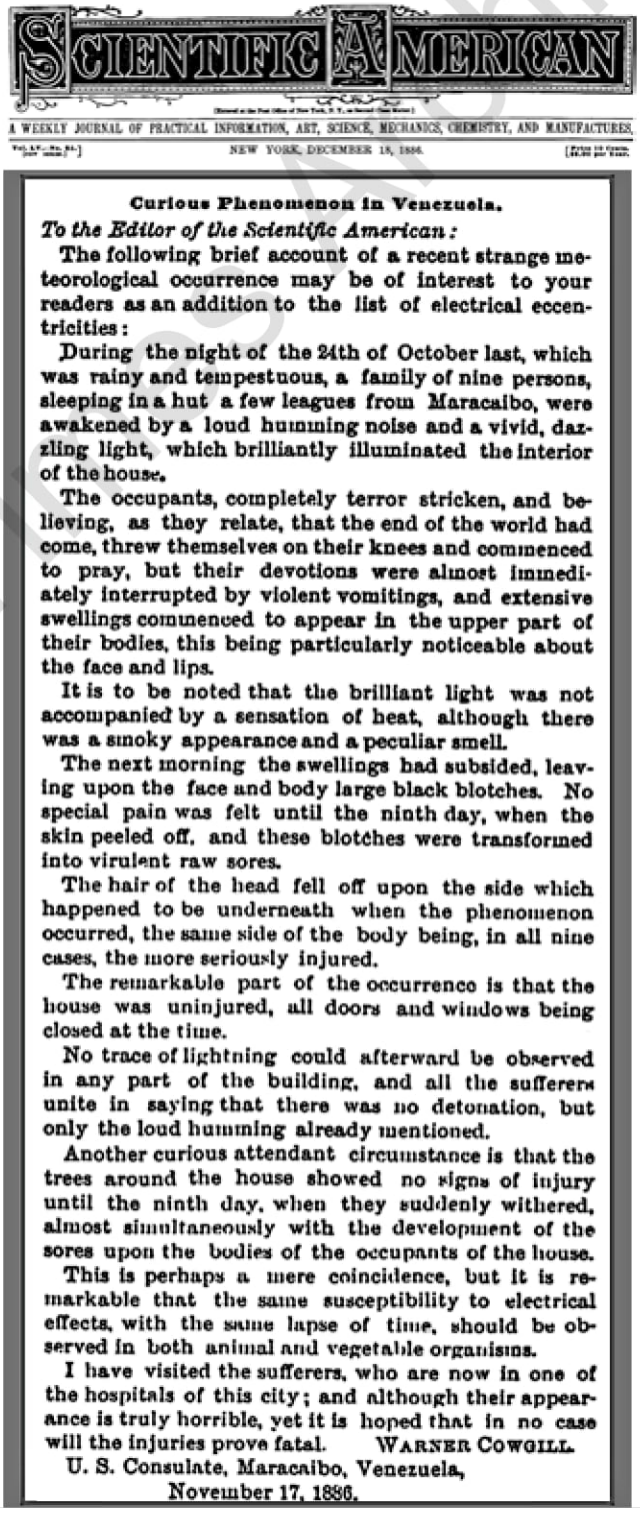
The internet data (purportedly) consisted of the text of a letter to the editor of *Scientific American* magazine, dated November 17, 1886, from a man named Warner Cowgill, at the U.S. Consulate, Maracaibo, Venezuela. This reference made the verification process very simple. I called the University of Oregon Science Library to see if their collection had issues of *Scientific American* from 1886. I was told their collection went back even earlier than that. I went to the Science Library, found the December 18, 1886 issue (the magazine was originally published weekly) and checked the Correspondence section. Indeed, the Cowgill letter was there on page 389. The internet report was completely accurate (save for a minor typographical error) and had quoted the letter in its entirety. With the library staff's assistance (the old magazine was quite fragile), I obtained about a dozen photocopies of the page containing the letter.

The letter (framed, this page) contains specific details regarding the events in question, and the medical condition of the victims.

There is a wealth of detail in this account, and it is worth examining the text carefully. Beginning with the first sentence, we see that Cowgill refers to the events as a "meteorological occurrence." This is not surprising. As noted above, radioactivity and X-rays would not be discovered for another nine years. Cowgill attempts to fit the information he has into the scientific framework with which he is apparently familiar. He makes no attempt to sensationalize events he clearly finds remarkable.

In the third, fourth, fifth, and sixth paragraphs we get the bulk of the medical information, which is crucial to this discussion. I phoned Donald Elting, the radiation safety specialist at the University of Oregon's Office of Environmental Health and Safety. [Mr. Elting graciously provided his assistance in my investigation of this information. Additional advice and reference materials were provided by Saul-Paul Sirag and the staff at the University of Oregon Science Library. The conclusions drawn (and any errors herein) are solely my own. Thanks also to Stuart Roach for asking tough questions.] I read to him the description of the injuries, without at first telling him the date of the letter. He confirmed that the symptoms—vomiting, tissue swelling (particularly about the face and lips), skin blotches, hair loss, and skin sores—are a clear indication of severe radiation exposure. He estimated the level of exposure to be 200 to 300 rads (radiation absorbed dose). This appears to be a conservative estimate. The fact that the victims began vomiting almost immediately may indicate a much higher level of exposure. In particular, Daniel A. Gollnick, in *Basic Radiation Protection Technology,* indicates that the onset of vomiting within thirty minutes of an exposure puts the exposure in the lethal range of ten to fifty (or more) Sieverts. (The Sievert is a newer measure of radiation injury that focuses on the amount of *cellular damage,* rather than on the amount of radiation absorbed, since different types of radiation vary in their biological destructiveness.) This converts to between 1,000 and 5,000 rems.[2] The rems then convert to between 1,000 and 5,000 rads of gamma radiation, or 100 to 500 rads of the more biologically destructive fast neutron radiation (using the Nuclear Regulatory Commission's "Q" conversion factor).[3]

In general, the delay in the manifestation of radiation-induced cellular damage is inversely related to the severity of the exposure, but some tissues consistently show damage



SCIENTIFIC AMERICAN

A WEEKLY JOURNAL OF PRACTICAL INFORMATION, ART, SCIENCE, MECHANICS, CHEMISTRY, AND MANUFACTURES.

NEW YORK, DECEMBER 18, 1886.

**Curious Phenomenon in Venezuela.**

*To the Editor of the Scientific American:*

The following brief account of a recent strange meteorological occurrence may be of interest to your readers as an addition to the list of electrical eccentricities:

During the night of the 24th of October last, which was rainy and tempestuous, a family of nine persons, sleeping in a hut a few leagues from Maracaibo, were awakened by a loud humming noise and a vivid, dazzling light, which brilliantly illuminated the interior of the house.

The occupants, completely terror stricken, and believing, as they relate, that the end of the world had come, threw themselves on their knees and commenced to pray, but their devotions were almost immediately interrupted by violent vomitings, and extensive swellings commenced to appear in the upper part of their bodies, this being particularly noticeable about the face and lips.

It is to be noted that the brilliant light was not accompanied by a sensation of heat, although there was a smoky appearance and a peculiar smell.

The next morning the swellings had subsided, leaving upon the face and body large black blotches. No special pain was felt until the ninth day, when the skin peeled off, and these blotches were transformed into virulent raw sores.

The hair of the head fell off upon the side which happened to be underneath when the phenomenon occurred, the same side of the body being, in all nine cases, the more seriously injured.

The remarkable part of the occurrence is that the house was uninjured, all doors and windows being closed at the time.

No trace of lightning could afterward be observed in any part of the building, and all the sufferers unite in saying that there was no detonation, but only the loud humming already mentioned.

Another curious attendant circumstance is that the trees around the house showed no signs of injury until the ninth day, when they suddenly withered, almost simultaneously with the development of the sores upon the bodies of the occupants of the house.

This is perhaps a mere coincidence, but it is remarkable that the same susceptibility to electrical effects, with the same lapse of time, should be observed in both animal and vegetable organisms.

I have visited the sufferers, who are now in one of the hospitals of this city; and although their appearance is truly horrible, yet it is hoped that in no case will the injuries prove fatal. WARNER COWGILL.

U. S. Consulate, Maracaibo, Venezuela,
November 17, 1886.

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sooner than do others. Cells that reproduce slowly, though damaged, may continue to function somewhat normally until such time as they would ordinarily undergo division. At that time, the cells fail to divide properly, and their functions become compromised. This makes the gastrointestinal tract a significant marker regarding radiation exposure. The cells in the gastrointestinal epithelium normally reproduce at a rapid rate, and any cellular damage therein becomes apparent very early on, compared to that of most other body tissues. That is why the "almost immediate" onset of "violent vomitings" is suggestive that this may have been radiation injury of an extreme nature.[4]

The fact that the victims felt "no sensation of heat" is significant. It is consistent with radiation injury. Even extreme radiation exposure produces no sensation of heat.[5]

The fact that the injuries are worse on the *underside* of the body is a remarkable part of the data. One would normally expect that a part of the body *farther* from a source of radiant energy would be injured *less* severely. (If you hold your hand too close to a fire, for example, the part of the hand *closest* to the fire will sustain the greatest injury.) Mr. Elting explained to me that the pattern of injuries is understandable. The radiation would have passed *through* the bodies, causing damage as it did so, and then bouncing off of the ground, would *re-injure* the tissues nearest the ground, and farther from the radiation source.

The fifth paragraph notes a nine-day time delay between the appearance of the skin blotches and their replacement with "virulent raw sores." Again, the delayed onset of some symptoms is consistent with radiation injury, with variation depending on exposure level, the particular tissues involved, and the particular type of radiation.

Paragraph seven notes that the house was not visibly damaged by the energy source. This description, too, is consistent with what we know about radiation exposure. Mr. Elting told me that this would likely indicate gamma and/or neutron radiation. (It is common knowledge that the controversial neutron bomb would leave buildings intact, while killing the occupants. I raise this as a point of reference for the reader, regarding neutron radiation. I am *not* suggesting that a neutron bomb caused the events. Cowgill specifically states that there was *no* detonation. Gamma radiation, in a manner similar to that of X-rays, mostly passes through things like the walls of houses.)

Paragraphs nine and ten, regarding the tree damage manifesting concurrently with the appearance of skin sores on the victims, appear consistent with radiation exposure. Not all cellular damage is immediately apparent in humans, and the delayed manifestation of injury to plants may follow in a similar fashion. However, the responses of plants to radiation are complex and varied. In some cases, irradiation has been known to *stimulate* growth. More study will be necessary on this point.[6]

Since the events near Maracaibo suggest anomalous, severe radiation exposure, we need to consider possible explanations for the events.

In the second paragraph, Cowgill recounts how the occupants were "awakened by a loud humming noise and a vivid dazzling light." As the reader may know, the bright light is a common characteristic of reports of Unidentified Flying Objects.[7] The humming noise is another such feature, though somewhat less common.[8] The "peculiar smell" mentioned in paragraph four of the letter, which possibly indicates the presence of ozone, is yet another (less common) feature of UFO reports.[9] And of particular importance to the discussion here, there are reports of serious radiation injury to humans, resulting from close encounters with UFOs.[10] It is therefore not unreasonable to consider an Unidentified Flying Object as a potential source of the energy. No vehicle of any sort is mentioned in Cowgill's letter, so we cannot conclude that it constitutes a "flying saucer" report. Nevertheless, there is sufficient similarity to warrant consideration of a UFO explanation.

We should also consider natural phenomena as possible sources. A volcanic or geological explanation seems wholly inadequate. Maracaibo is in a lowland area, near the Gulf of Venezuela. It is about forty-five miles from any significant mountains, and hundreds of miles away from the nearest volcano, Mt. Tolina in Columbia.[11] More to the point, the victims felt no sensation of heat when they were injured, and the injuries themselves clearly were not ordinary burns.

Radioactive ores, even if there were any present, could not possibly be radioactive enough to cause the reported injuries.

On the other hand, the phenomenon of ball lightning bears a number of striking similarities to the events near Maracaibo. According to *Scientific American's* internet website "Ask the Experts":

1) Ball lightning usually appears during thunderstorms.[12] (Cowgill says the night in question was "rainy and tempestuous.")

2) It has been known to pass through closed windows, with little or no damage to the glass.[13] (Cowgill says all the doors and windows were closed.)

3) It occurs in flatland areas, and not in mountainous regions.[14]

4) The smell of ozone and nitrogen oxides has been reported in conjunction with the phenomenon.[15] (Cowgill says, "There was a smoky appearance and a peculiar smell.")

5) Ball lightning usually produces no discernible heat.[16] (This is consistent with Cowgill.)

Does this solve the riddle? Not entirely. While there are significant parallels between the Maracaibo events and the phenomenon of ball lightning, there are a number of inconsistencies as well:

1) Ball lightning reports do not include descriptions of humming noises.[19]

2) According to Peter H. Handel, "The lifetime of ball lightning tends to increase with size and decrease with brightness."[17] The phenomenon near Maracaibo lasted long enough to wake nine persons from sleep (with the unexplained humming noise), and then seriously injure them. Yet the light was described as "vivid, dazzling." This is inconsistent with Handel's assessment—a bright ball should not have lasted so long. And Lowke says that ball lightning typically is only as bright as a 20-watt incandescent bulb—hardly "dazzling."

3) On the other hand, Martin Uman writes that there is a rare type of ball lightning that is unusually bright and long-lived, and that it probably accounts for some UFO reports.[18] This, of course, strengthens the ball lightning explanation somewhat, and mitigates the problem implied by Handel's statement. Ball lightning is itself the subject of some controversy.

4) Handel says, "Open-air ball lightning often ends with a violent explosion." The Maracaibo victims said there was "no detonation." (This is perhaps problematic, but is not a fatal flaw, since the victims were injured inside their home.)

5) Lowke and Handel say that ball lightning has been observed inside houses, submarines, and airplanes.[20] In particular, Handel (a proponent of the so-called Maser-Soliton Theory to explain ball lightning) says that the energy level of ball lightning in confined spaces such as these would be limited to about 10 joules—"too little to be dangerous to life."[21] But the Maracaibo victims were severely injured *inside* their hut—with the doors and windows closed.

6) Martin A. Uman says ball lightning has produced "no reasonably documented cases of injury or death. Lots of close brushes with no injury."[22] This, of course, is the greatest problem with the ball lightning explanation. While ball lightning seems to vary greatly in size, brightness, duration, tendency to explode, temperature, and sonic properties, *it has never been known to cause radiation injury.*

So the riddle remains. It is perhaps possible that under certain rare circumstances ball lightning could produce injurious radiation. That would be the simplest solution, and it may, in fact, be correct. It would then account for the observed weather

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effects and for the radiation injuries. (Occam advised against "unnecessary complication.") But more research into ball lightning will be necessary before this explanation for the Maracaibo events can be regarded as correct. There is currently no generally accepted theory as to the cause of ball lightning.

Another possibility is that certain UFOs use propulsion systems that produce effects similar to those of ball lightning. Again, the humming noise and apparent radiation injuries reported by Cowgill are features of UFO reports, not of ball lightning reports. Presently, there appear to be problems with both explanations and no scientific basis for choosing either one.

I find it ironic that Lowke accepts anecdotal evidence for the existence of ball lightning (apparently with the approval of the editors at *Scientific American*), asserting that competent observers are reliable sources for data:

> Although there is at least one textbook on lightning that questions the existence of ball lightning, and I have never seen the phenomenon personally, I feel that there is no question that ball lightning exists. I have talked to six eyewitnesses of the phenomena and think there is no reasonable doubt as to the authenticity of their observations. Furthermore, the reports are all remarkably similar and have common features with the hundreds of observations that appear in the literature.[23]

The irony is that UFO literature is *saturated* with statements virtually identical to Lowke's, from competent researchers regarding reliable UFO eyewitnesses. And yet the study of UFOs is often portrayed as a pseudo-science. Consistency among ball lightning reports is presumed to result from the presence of an actual phenomenon. Consistency among UFO reports is presumed to be the result of misperception, confabulation, witness contamination, mass hallucination, or fraud.[24] This double-standard for assessing the validity of data is decidedly unscientific. The Maracaibo report has aspects consistent with both phenomena. Can the same data be considered reliable where it points to ball lightning and unreliable where it points to UFOs? Both are elusive phenomena. UFOs cannot be produced at will, and ball lightning has never been synthesized in a laboratory.

### Conclusion

In my view, the Cowgill letter is an extraordinary document and represents a true scientific anomaly. Even if Cowgill fabricated the report (a theory for which there is no evidence whatsoever) we would still be left with the mystery as to *how* a person in 1886 could possibly have known in such thorough detail about the medical effects of a then unknown energy source. It is simpler to take Cowgill at his word. But then we are left with the mystery of the events themselves.

Additional study may yield more data. I am attempting to get biographical information on Cowgill, though it really won't make any difference whether he was an ambassador or a waiter. His report was meticulously done and stands on its own merit. I may also be able to find further accounts of the fate of the victims, either in newspaper accounts, or in hospital records (with the help of someone to translate the Spanish).

Finally, it is my hope that as the Cowgill letter becomes more widely known, the scientific inquiry into anomalous data will be considered in a more even-handed fashion and less subject to the very unscientific practice of ridicule. The information is from an unimpeachable source. We should approach it with honesty, not denial.

### References


**1.** Segre, E. 1980. *From X-Rays to Quarks*, W.H. Freeman & Company, San Francisco, 21, 28-29.

**2.** A 1 Sievert exposure is equivalent to 100 rems (per Author's conversation with Donald Elting).

**3.** Gollnick, D.A. 1994. *Basic Radiation Technology* 3rd Edition, Pacific Radiation Corporation, Altadena, California, 96. Radiation exposure data are variously expressed in rems, rads, sieverts, and other units. The conversion of rems to rads is complex. It depends on the type of radiation and the conversion ("Q") factors themselves vary significantly, from one regulatory agency to another. Some Q factors would make the lethal dose for fast neutrons as low as 40 to 200 rads.

**4.** Becciolini, A. 1991. "Radiosensitivity of the Small and Large Intestines," *Encyclopedia of Human Biology*, Volume 6, Academic Press, San Diego, 469-480.

**5.** Elting, D. Personal communication to author. Elting points out that nuclear bomb blasts do produce heat as well as radiation. But Cowgill specifically states that there was no detonation; the heat effects of such weapons are not relevant to this case.

**6.** Leavitt, J. 1972. *Responses of Plants to Environmental Stresses*, Academic Press, New York, 460-463.

**7.** Hynek, J.A. 1972. "Nocturnal Lights," *The UFO Experience*, Henry Regnery, Chicago, 36-51.

**8.** Fowler, R. 1981. *Casebook of a UFO Investigator*, Prentice Hall, Englewood Cliffs, NJ, 45.

**9.** *Ibid.*, 72.

**10.** Vallee, J. 1990. *Confrontations*, Ballantine Books, New York, 113. Vallee describes a number of UFO radiation injury cases, including that of Steven Michalak, whose close encounter with a UFO occurred eighty miles east of Winnipeg on May 20, 1967. Vallee says, "Michalak was treated at Misericordia Hospital for chest burns. In the following weeks he suffered from constant nausea, vomiting, and diarrhea and a drop in his white blood cell count." The similarity to the Maracaibo account is obvious. Vallee is an astrophysicist, with a doctorate in computer science. I consider him to be among the best UFO researchers.

**11.** *Rand McNally Cosmopolitan World Atlas*, Rand McNally & Company, Chicago, 1962, 53 and 60. The age of this atlas (which has been in the author's possession since 1964) puts it closer than a modern atlas to the events of 1886 under consideration.

**12.** *Scientific American* internet web page. 1999. http://www.sciam.com/askexpert/physics/physics30.html, December 4, citing work by Dr. M. Uman, astrophysicist P.H. Handel, plasma physicist J. Lowke, and others.

**13.** *Ibid.*

**14.** *Ibid.* (Peter H. Handel)

**15.** *Ibid.* (John Lowke)

**16.** *Ibid.* (John Lowke)

**17.** *Ibid.* (Peter H. Handel)

**18.** *Ibid.* (Peter H. Handel and John Lowke)

**19.** Uman, M.A. 1986. *All About Lightning*, Dover Publications, New York, 133.

**20.** *Ibid.*, 123-133. Uman cites reports of "buzzing" and "hissing" noises associated with ball lightning, but no loud humming. Many reports describe the ball as having made no noise at all.

**21.** Handel, P.H. *Scientific American* internet web page.

**22.** Uman, M.A. 1999. Chair, Department of Electrical and Computer Engineering, University of Florida at Gainesville. Email to the author, December 6. Dr. Uman has published dozens of papers on electrical phenomena and is one of the experts cited on *Scientific American's* web page. Dr. Uman graciously answered my query into whether ball lightning had ever been known to produce ionizing radiation. The conclusions I draw from his response are solely my own.

**23.** Lowke, J. *Scientific American*, internet web page.

**24.** Read almost anything on UFOs from any member of the *Committee for the Scientific Investigation of Claims of the Paranormal*.

**About the Author**

Gordon D. Kaswell composes and records music in Eugene, Oregon. His compositions have been featured on the ABC, NBC, and Cinemax networks, and in Europe. He wrote and produced the soundtrack for the award-winning video *Power of the Group*. He attributes his interest in science and the politics of science to the influence of textile chemist Ernest R. Kaswell, his father.

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## Response from James P. Kornberg, M.D., Sc.D.,* to "Phenomenon in Venezuela" Article

I have reviewed the paper "Phenomenon in Venezuela: A Documented Case of Unexplained Radiation Exposure," submitted by Gordon David Kaswell.

I have found Mr. Kaswell's paper and the incident it describes very fascinating and thought-provoking. With respect to Mr. Kaswell's commentary and interpretation, I offer the following:

1) The "violent vomiting" described to have occurred shortly after the occupants were awakened by the "loud humming



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noise" and the "vivid, dazzling light," requires explanation. Mr. Kaswell correctly notes that victims of whole body radiation exposure who begin vomiting "almost immediately" may have been exposed to radiation much in excess of 200 to 300 rads. Quoting his reference, he indicated that "the onset of vomiting within thirty minutes of an exposure puts the exposure in the Lethal Range of 10 to 50 (or more) Sieverts."

Having said that, Mr. Kaswell does not reconcile his observations with the apparent survival of all "exposed persons" for at least twenty-four days (October 24 until November 17).

The gastrointestinal form of the acute radiation syndrome usually occurs between exposures of 600-1000 rads or greater (*Dalrymple Medical Radiation Biology*, Saunders, Philadelphia, 1973, p. 193). Under these circumstances, the prodromal signs of vomiting usually occur within a few hours after exposure. After twenty-four to forty-eight hours, the patient usually enters the "latent phase," during which time he or she may feel better. The latent phase may last about one week, after which time the patient will usually re-experience severe nausea, vomiting, and diarrhea with fever that usually progresses to bloody diarrhea, shock, and death within ten to fourteen days.

Now, we apparently do not know whether any or all of the nine "exposed" persons survived; but Mr. Cowgill was optimistic enough to state on November 17 ". . .although their appearance is truly horrible, yet it is hoped that in no case will the injuries prove fatal."

Whole body radiation exposure sufficient to have induced vomiting within the time frame described would have probably led to the deaths of one or more of the family members by the time Mr. Cowgill visited them in the hospital. Mr. Kaswell, therefore, should probably not recruit the "onset of acute vomiting" to support his assertion of acute radiation exposure.

If we assume that the family members vomited within the time frame described, therefore, we must conclude either that they were exposed to much lower levels of radiation and/or that they vomited for a different reason. If they were exposed to much lower levels of radiation, they would have probably been exposed in the range of 200-500 rads. This level of radiation, in fact, can lead to the cutaneous manifestations of acute radiation exposure that are much more consistent with the described clinical course of the family members.

Well, then, what about the vomiting? The family members were described as being "completely terror stricken." Believing that the end of the world had come, they "threw themselves on their knees and commenced to pray." Only then were "their devotions. . .almost immediately interrupted by violent vomitings. . ." Clinically, I suggest that one or more of these persons vomited because they were terror stricken. When a person is terrified, there is often a tremendous autonomic nervous system overflow that can predictably lead to vomiting in many individuals. Most of us have had the experience that when one sees, hears, or smells another person vomit—you guessed it—an epidemic of vomiting ensues! I think that this latter mechanism better explains Cowgill's report. As mentioned, however, this explanation does not rule out the possibility of contemporaneous exposure to radiation at levels sufficient to cause the cutaneous effects described.

2) In order to explain the "distal from source" or "closer to the ground" pattern of injury, Mr. Kaswell relies upon Mr. Elting's explanation of reflected radiation causing re-injury of affected tissues. I find this to be somewhat far-fetched, especially since Cowgill also noted that, "The hair on the head fell off upon the side which happened to be underneath when the phenomenon occurred. . ." Hair loss following whole body radiation exposure is usually symmetrical, in my experience. Even given Elting's explanation, I find it hard to believe that the gradient of exposure would have been sufficient to cause hair loss on one side and not the other. I simply accept that this observation remains unexplained. The phenomenon is certainly consistent with radiation exposure but should not be over-utilized to support that hypothesis.

3) Mr. Kaswell acknowledged that it is possible that "under certain rare circumstances ball lightning could produce injurious radiation." Just after reminding us that more research into ball lightning will be necessary before regarding this possibility as being correct, he launches into the "possibility. . .that certain UFOs use propulsion systems that produce effects similar to those of ball lightning." It is at this point that I became very confused. Having performed a commendable job in scientifically analyzing this event up until this point in his article, Mr. Kaswell then uses "certain" UFOs (!) to explain the production of effects similar to ball lightning which he just stated is an under-studied phenomenon! Like a UFO, this reference to UFOs comes out of nowhere and throws very cold water upon a hitherto good effort. I am by no means a UFO debunker, but up until this point in the article, I was still fascinated by what does appear to be an unexplained, rather well-described human radiation exposure event. I do not need an unexplainable UFO to describe an unexplainable, possible radiation exposure event.

Mr. Kaswell soberly invokes the presence of "certain UFOs" to bring "it all together." Mr. Kaswell then proceeds to describe the controversy between "ball lightning believers" versus "UFO believers," obviously siding with the latter. What happened to our sick Venezuelans? Having blasted the UFO debunkers, Mr. Kaswell then returns again to the subject at hand by presenting some coherent conclusions, at least up until his last paragraph when he gets his licks in again, by alluding to the "unscientific practice of ridicule." Who is ridiculing? What does a debate between "ball lightning believers" versus "UFO believers" have to do with the events described by Cowgill?

For unexplained reasons, Mr. Kaswell also regards Cowgill as an "unimpeachable source" who filed a "meticulous" report. How did Mr. Kaswell reach those conclusions? I have no reason to believe that Cowgill is unimpeachable, and his report was far more general and anecdotal than it was meticulous. It was just a letter to the editor of *Scientific American*. Cowgill may or may not have reported all that he knew, so how do we know that he was as meticulous as Mr. Kaswell credits him to have been?

4) Until invoking a distracting and annoying personal bias against UFO debunkers, Mr. Kaswell was on his way to a good paper, having obviously performed research, including the seeking of expert opinions, to explain a genuinely interesting and unexplained event that is consistent with some aspects of human whole body radiation exposure.

5) In closing, I guess I should not be too hard on Mr. Kaswell. He has raised some interesting issues and I share his fascination with Cowgill's report. I simply take Cowgill's letter for what it probably was, simply a letter describing an interesting, unexplained event. As a student of medical history, I have encountered many historical, clinical descriptions that I am inclined to diagnose from the distance of decades or even centuries. I just try to avoid relying upon UFOs or wizards to explain those probably, readily explainable medical maladies.

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## Gordon D. Kaswell's Response to James P. Kornberg

Dr. Kornberg raises some worthwhile points regarding particular aspects of the Cowgill letter. However, he seems to have misconstrued the thrust of my arguments regarding possible explanations for the events under consideration.

I am, of course, pleased that Dr. Kornberg considers the case for radiation exposure to be strong enough that it warrants discussion of the finer points of radiation levels and injury. His point is well-taken that the date of the letter may imply that the



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victims survived for twenty-four days after they sustained their injuries, and consequently had a level of exposure somewhat lower than my own estimate, though Cowgill did not specify when and how often he monitored the victims' status. Dr. Kornberg's estimate of 200-500 rads exposure level is more in keeping with that of Donald Elting. I do wish that Dr. Kornberg had used the Sievert units since, as I pointed out in my paper, not all rads indicate the same degree of injury, the Q factors having been established just for this reason. Of course, the basic facts of the case remain; the radiation injuries are anomalous.

Dr. Kornberg "clinically" suggests that the almost immediate onset of vomiting was due to terror and a kind of "epidemic of vomiting." I do not know what he means specifically by the term "clinically." If clinical work has been done on causes of vomiting, he should cite the particular work or study. Perhaps he simply refers to his own work or opinion. The "epidemic" spread of vomiting familiar to "most of us" occurs in ordinary life, not under conditions of extreme terror. If Dr. Kornberg knows of specific evidence (anecdotal or otherwise) that demonstrates this effect under conditions of terror, he should cite it. And again, whether or not the vomiting can be viewed as evidence of an extreme level of exposure, the victims' injuries are still unexplained.

Dr. Kornberg is skeptical of Donald Elting's reflected radiation explanation for the "closer to the ground" pattern of injury, though he offers no other. He does not dispute that the hair loss is consistent with radiation exposure.

I find Dr. Kornberg's reactions to my observations regarding the similarity of the Cowgill letter to modern UFO *reports* quite puzzling. I *never* stated that UFOs were the cause of the Maracaibo injuries. I pointed out that a number of aspects of the Cowgill letter were consistent with those of modern UFO reports. Perhaps he missed my reference in the footnotes to the Steven Michalak case. Michalak's reported nausea, vomiting, chest burns, diarrhea, and lowered white blood cell count—symptoms obviously similar to those of the Maracaibo victims—were reportedly the result of a UFO encounter. If numerous aspects of the Maracaibo report resemble the Michalak case and other UFO reports, it is absolutely reasonable to point this out. Contrary to Dr. Kornberg's assertion, the UFO reports cited in my paper did not "come out of nowhere."

Dr. Kornberg seems to think that my paper is an attempt to resolve a supposed debate between what he euphemistically refers to as "ball lightning believers" and "UFO believers." In so doing, he completely misses my point. My purpose was (in part) to show that mainstream scientists in mainstream publications like *Scientific American* evaluate the largely anecdotal evidence for the relatively non-controversial ball lightning by a different and more lenient standard than that they reserve for the highly controversial UFO reports. The Maracaibo case illustrates this beautifully, precisely because the report has aspects similar to both phenomena.

Dr. Kornberg is unaccountably critical of Cowgill's reliability (though he refers to the events in question as being "well-described"), and questions my confidence in Cowgill's report. My confidence is based on the fact that Cowgill reported the condition of the victims with sufficient thoroughness that over a hundred years later, even Dr. Kornberg accepts the possibility their injuries were radiation-caused. But Cowgill did better than that: He reported on the date of the injuries, the weather conditions on the night in question, the light, the noise, the condition of the house, the condition of the surrounding vegetation, and the chronology of the damage to both the people and the vegetation. All this from a person completely naïve regarding the nature of ionizing radiation. The report is unimpeachable in that there is no realistic possibility of fraud, since (as I stated in my paper) the date of the publication precedes by nine years Roentgen's discovery of X-rays, and the original *Scientific American* issue is readily available in any good science library.

Despite his protestations that he is "by no means a UFO debunker," Dr. Kornberg employs faulty logic in a manner that is the debunker's stock and trade. In stating that he "just [tries] to avoid relying upon UFOs or wizards to explain those probably readily explainable medical maladies," he attempts to discredit the UFO aspects of my paper by implying an equivalence between UFOs and "wizards." "Wizards" are, of course, completely irrelevant to this issue, and such obfuscations are contrary to good science and free inquiry. I do not share Dr. Kornberg's confidence that the Maracaibo injuries are "readily explainable." Presumably if he had such an explanation he would have stated it in his assessment. Despite his implied favoring of the ball lightning explanation and the hypothetical possibility that ball lightning could cause radiation injury, the data we have at the present time do not support this view.

▲▲▲

## Apex Resort Hotel – ICCF6

The ICCF6 conference in Hokkaido was held at the Apex Resort Hotel, located at the top of a mountain in the middle of nowhere. The building was constructed at the height of the "bubble" economy. Inside it resembled a Hollywood sound stage, reeking of money. Outside was a golf course, a ski lift, and miles of access roads. It must have cost millions. It soon became a typical 1980s white elephant. The view from the lobby was spectacular, but the hotel was inaccessible and overpriced, and it lost money from the start. Foreclosure followed foreclosure, and the deed to the property bounced around from desperate real estate developers with alleged mob connections, to banks which went bankrupt, to a quasi-public prefectural corporation. The hotel was already bankrupt during ICCF6, and some said the conference organizers drove a hard bargain with the management to get a cheap deal on the rooms. Soon after the conference, the hotel shut down for good and was abandoned.

The hotel was recently in the news again, in the Yomiuri newspaper, April 9, 2000. Early in April a volcano three kilometers away began erupting. The building has huge glass picture windows facing the volcano, and it is positioned about the crater, looking down. It would be an ideal place to observe the eruption, were it not for the danger of being asphyxiated or crushed by falling rocks. The Yomiuri reporter expressed "some anxiety" about that. A photograph shows several reporters ensconced in the gigantic lobby, awake all hours of the day and night looking out of the window through camera viewfinders. The article reads like the diary of the reporter, describing surrealistic living conditions in the ghostly luxury hotel, which now has no electricity or heat and only one working toilet. The reporters are on a camping trip inside a luxury hotel. They are "too exhausted to talk" and they eat mostly chocolate emergency rations. Old dirty wine glasses and silverware were left on the tables when the building was deserted. The article does not say whether any volcanologists were holed up with the reporters. Perhaps volcanologists have more sense than to camp out a few kilometers away from an eruption. It sounds a little like being marooned in a luxury villa in Pompeii in A.D. 79.

As of June 5, there has been no major eruption. Perhaps the reporters have gone home, and hotel has been abandoned once again. —*Jed Rothwell*

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# New Energy Research Laboratory

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# Device and Process Testing Update

Ed Wall and Ken Rauen

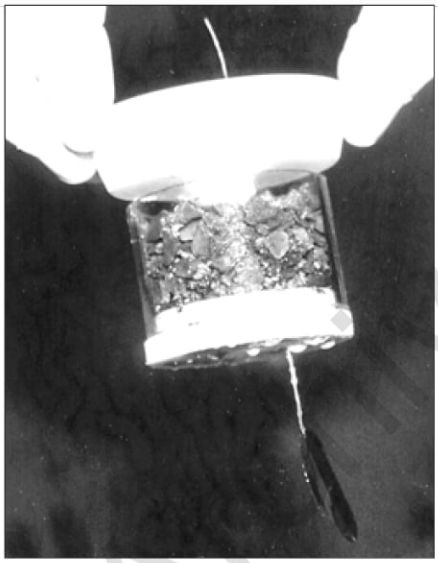
### Hydrosonic Pump

NERL's testing using a circulation pump, flow rate meter, inlet and outlet thermocouple temperature readings, and radiator, as reported in the last issue, was not working well. When it was discovered that the Hydrosonic Pump would itself pump about 7 GPM without the external circulation pump, a means of testing without a circulation pump and flow metering emerged. The new method is very simple: raise the temperature of a fixed quantity of water by a measurable amount and compute the change in enthalpy, then compare this to the input energy measured.

We now have two large plastic barrels (~ 65 gallons each) glued end-to-end, with four thermocouples mounted in the side at different levels. Enough water fills the assembly such that a run was expected to be long enough for good calorimetry, but then, something else unexpected occurred. The intake for the Hydrosonic Pump had water fed into it that was pressurized by a head pressure of about five feet. We were expecting the Hydrosonic Pump to run in liquid mode, that is, we did not expect any steam production. But the motor started drawing about 87 kW, which is far above what is expected for the 75 HP motor (56 kW). We verified our readings and started worrying that we might destroy the motor if we kept running it.

We were disappointed, but relieved that the cause of an excessive current draw in our Hydrosonic Pump runs was due to a design error, originating with HydroDynamics. President Kelly Hudson is dealing with this problem in a very responsible way. The company will be sending NERL a new pump (rotor) assembly to replace the one we have now, which is oversized for the motor. Recent information from the company is that we may receive the replacement in as little as two weeks.

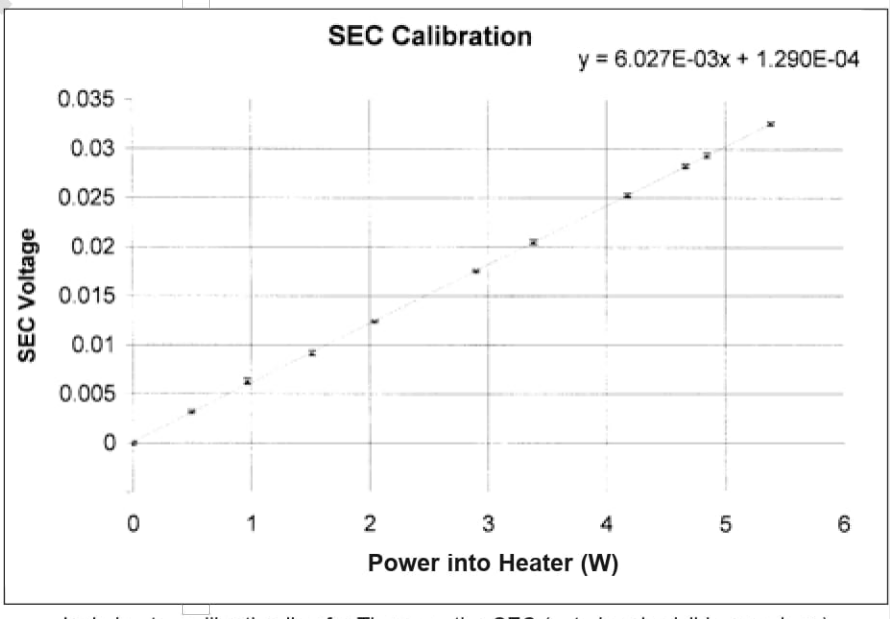
We decided to run the Hydrosonic Pump in steam mode by restricting the water intake. We were able to get it to boil off the contents of the pump itself, but when we opened the intake valve a bit, very little water flowed. When we opened it more, the water would surge in, causing severe shuddering, suddenly increasing power consumption from 7 kW to 87 kW, then stop flowing as steam pressure built, until the steam came out and more water would surge into it, repeating cyclically. No matter how the intake valve was set, the surging problem continued. The mechanical stress on this "behemoth," when the shuddering happened is seriously non-trivial, so we have decided to discontinue testing until the properly sized replacement unit arrives.



Dash/Warner Cell cap with recombiner holder.

### Dash Cell Progress

Jon Warner (at Portland State University, Professor Dash's doctoral student) presented his cold fusion results at ICCF-8 in a clear and concise fashion. We will be reporting more on Warner's work in the future. He mentioned to Ed Wall that (at Ed's suggestion) he had tried moving his cell assembly to the corner of the Seebeck Envelope Calorimeter (SEC) and found a noticeable change in the SEC output voltage for the same power to the Device Under Test (DUT). This is due to conduction of heat through the wall of the calorimeter to the area in which the reference thermocouples are located, perturbing the reference temperature. Of course, if he keeps his test and calibration cells in the same place for all runs, this should not present a big problem, but it is better not to have the warm cell in direct contact with the inside calorimeter surface. Ed Storms recommends inserting a small fan into the SEC to avoid hot spots. This would help with the problem of hot spots, and allow for the cell to dissipate heat and run with more input power without reaching boiling, but it would decrease the sig-




Joule heater calibration line for Thermonetics SEC (note barely visible error bars).

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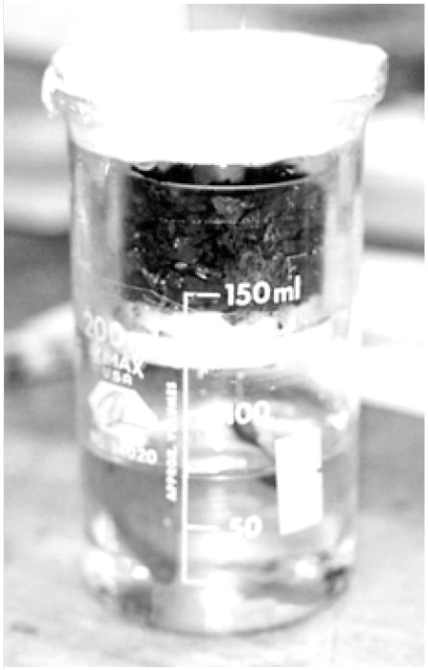
NERL's test and calibration cells. Note the large space for holding re-combiner (United Catalyst palladium-coated carbon).




Dash/Warner Cell ready for a run.

nal-to-noise ratio of the SEC. For now, we are setting the DUT in NERL's Thermonetics SEC on a small platform to allow air convection on all sides.

The Thermonetics SEC has been in use for some time now. Initially, we did not have a room with good temperature regulation to compensate for the cool nights and warm afternoon sun. We now have a room with air conditioning and heating that is probably as well-regulated as that found in most office or lab environments. Even with this enhancement, however, fluctuations in SEC output voltage on the order of 1 mV (which corresponds to ~ 150 mW DUT power variation) were seen when the air conditioning or furnace cycled. We then built a foam box around the SEC, but this only slightly attenuated the perturbation caused by room temperature fluctuations. The next step was to use "active" insulation to reduce the effect on the SEC. The foam box has two large muffin fans in it to vigorously circulate air and the SEC is raised from the bottom of the foam box by spacers to allow circulation on all sides. It has a dispersed heater consisting of many resistors strung out in the foam box space. These resistors are connected to the output of a Watlow PID controller. The controller monitors the air temperature in the foam box and is tuned to the control characteristics of the box (easier than it seems). This has helped dampen effects of room temperature fluctuations considerably (~ 80%). More important, the improvement has drastically reduced the effect of long-term drift in ambient temperature, which seriously affects calorimeter accuracy, not just precision. We should probably mention that we had to put quite a few holes in the foam box to allow for heat exchange between the environment and SEC, in case a reader decides to try this. The idea of the SEC, after all, is to measure heat flux coming out of its envelope.

Although two test cells almost identical to Warner's were assembled months ago, they have not yet been used. The difficulty of the welding required to attach platinum leads to electrode foil pieces is the main cause of the delay. We had planned on using our small TIG welder, but it simply cannot deal with such thin metal pieces without destroying them. We now intend to use a small spot welder, which is what Dash and Warner use. The problem was that we did not have one. So, a requisition was forwarded to the "Department of Improvisation," and a couple of non-working spot welder prototypes emerged. Then the latest version was developed, which works better (large capacitance, lower voltage seems to do the trick).

**Peter Mobberley's Innovations**

An enthusiastic *IE* subscriber from the UK we met at ICCF8 announced to the audience after Dr. Mizuno's presentation on

***Testing Update continues on page 68***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# Renewable and Conventional Energy

## BUG POWER

**Microbes busy digesting a meal can supply renewable energy for the rest of us.**

by Bennett Daviss

Everyone knows that you get energy when you eat carbohydrates. But researchers are perfecting ways to get energy when someone else eats them. Scientists are making batteries and fuel cells to harness electricity generated by microbes as they eat.

These researchers predict that, properly harnessed, the energy released by the microscopic bugs as they nosh could eventually power laptop computers, cars, even entire Third World villages. "The potential," says Peter Bennetto, "is enormous."

Bennetto is in a position to know. Although work on microbial fuel cells is now spreading to other countries, Bennetto—an electrochemist at King's College in London—launched the research more than a decade ago. Bennetto and his colleagues fashioned a small plastic chamber divided vertically in half by a thin polymer film. On one side of the divide, a solution of ferricyanide or buffered oxygen roiled; on the other, nitrogen bubbled through a broth of organic chemicals—a buffet for microbes.

The idea didn't originate with Bennetto. Eighteenth century Italian physicist Luigi Galvani asserted an intimate relationship between electricity and living things, an idea he demonstrated by making severed frog legs jump by passing a current through them. In 1910, English botanist Michael Potter plunged a platinum electrode into solutions of yeast or E. coli and registered a twitch of current. But he never could explain its source. "The scientific community," says Bennetto, "was not overawed."

However, biologists now understand the electrochemical steps by which organisms disassemble carbohydrates into usable forms and parts. At the atomic level, where electrons are gained and lost, it's known as the reduction-oxidation or "redox" reaction.

As a carbohydrate molecule is taken apart, it loses electrons or, in chemistry's parlance, becomes oxidized. Typically, these electrons are seized by compounds that are constantly being created and consumed. These intermediate substances are said to be "reduced"—they absorb and hold loose electrons until the electrons reach their ultimate destinations in the metabolic process, usually oxygen or pyruvic acid.

That's the process that Bennetto set out to tap. As the single-celled bugs in his broth digest, the electrons they liberate flow through the soup to wires embedded in a carbon curtain hanging in the chamber. The wires carry the electrons through an external circuit and back into the other side of the cell, where they're absorbed by the oxygen or ferricyanide.

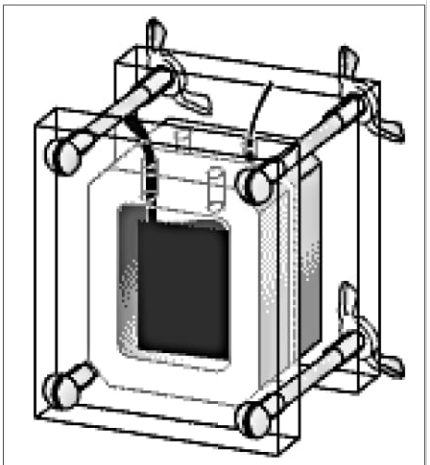
If the process sounds familiar, that's because "it's the conventional set-up for an electrochemical cell"—a flashlight battery, for example, says John Stirling, the team's biochemist.

***Thirteen gallons of a sugar solution could send a passenger car 600 miles, they calculate. The team foresees refrigerator-size bug cells able to churn out a steady kilowatt of power, enough to meet a sizeable share of the needs of a typical U.S. household.***

But there's a difference. According to Bennetto, a bug digesting its lunch turns a larger proportion of fuel into energy than do typical zinc- or lead-acid batteries. Indeed, the amount of energy a microbe can drain from its fuel can approach that of lithium, long considered an ideal (if pricey) battery material. In one experiment, a bug called *Proteus vulgaris* surrendered to an electron fully half of the electrons it loosed in digesting glucose. E. coli, a more industrious critter, converts more than ninety percent of a sucrose meal into carbon dioxide and water, sparking "an electrical yield approaching the theoretical maximum," Bennetto says.

The operative word here is "theoretical": the team's early cells could garner no more than a fifth of a watt. To boost the harvest, the group had to figure out a better way to collect the electrons—which normally are held by the redox reaction's reduced substances. The team decided to take the direct route: to find a way to reach inside the microbes and snag the electrons before the reduction products could.

The obstacle was the bugs' thick skins, a double layer of lipids (fat). The researchers cracked the problem by using a family of fat-loving, electron-hungry "mediators"—substances able to pierce the bugs' lipid layers like molecular needles, draw off the electrons, and spirit them back out to an electrode. If added to the organic broth in the right concentrations, the mediators allow the chemists to harvest enough electrons to



Assembled fuel cell

Courtesy NCBE website

New Energy Times Archive

sustain a current while leaving the microbes enough as well to stay alive, hungry, and reproducing.

In theory, such a living battery could run forever. In fact, when the team tested E. coli swimming in a pool of lactose, the bugs sustained a current for more than three months. At that point, the researchers turned off their equipment. "We'd proved the principle of microbial generation of electricity," Bennetto says. "We had other things to do."

One item on the to-do list is to figure out a way to siphon off microbes' waste. "Some of the waste, particularly hydrogen ions, accumulates to gradually make the solution acidic"—an environment not conducive to microbial life, Bennetto notes. Also, the microbes don't always clean their plates. "Some of the leftovers can physically stick to the electrodes and grunge up the works."

Once such problems are managed, the group expects to scale up quickly. Thirteen gallons of a sugar solution could send a passenger car 600 miles, they calculate. The team foresees refrigerator-size bug cells able to churn out a steady kilowatt of power, enough to meet a sizeable share of the needs of a typical U.S. household. "Our research indicates that a room-size reactor, containing a million liters of liquid and ten tons of micro-organisms, could produce a megawatt of power from 200 kilograms of carbohydrates per hour," Bennetto reports, enough to provide modest power for a small village. In sugar-producing nations such as Cuba and the Philippines, molasses and other cane-processing by-products are ready to be used as a feedstock, he adds.

For the immediate future, however, the team is thinking small. "Uses might be in miniature batteries for wristwatches and that sort of thing," Bennetto thinks. "A device that small could still contain enough feedstock and microbes to last a long time." One of the group's early prototypes was only an eighth-inch square but supplied enough power to run a digital clock for a day. Bennetto says, "Even one small cell containing less than a tenth of a gram of organisms is capable of driving a small motor."

Versions of the technology are beginning to pop up in the marketplace; breweries are using a biobattery to test the vigor of yeast, for example, and test units are available from the National Centre for Biotechnology Education at England's Reading University for 40 pounds sterling. (Details are available at www.eibe.reading.ac.uk/NCBE/MATERIALS/fuelcell.html.) But at that point, Bennetto and company begin to lose interest. "Now you're just talking about engineering," he says. "We're in this for the science."

**Bennett Daviss contributes to several national and international magazines including Discover, New Scientist, and Air & Space.**

## Texaco Buys 20% Stake in ECD, Including Mystery "Regenerative" Fuel Cell

Reprinted with permission from the *Hydrogen and Fuel Cell Letter*, June 2000.

*Detroit, MI*—With resistance to hydrogen crumbling among major American oil companies, Texaco, Inc. last month took a big step forward by buying one-fifth equity of one of the pioneers, Energy Conversion Devices, Inc. (ECD), of Troy, MI.

But more intriguing than Texaco's decision was what Texaco, third-ranked among America's big oil companies, was buying into for $67.3 million:

In addition to getting access to ECD's well-known, well-established proprietary Ovonic metals hydride storage technology, the joint announcement said Texaco, via its new subsidiary Texaco Energy Systems, Inc. set up last year to commercialize hydrocarbon-to-liquids and fuel cell technologies, would become partner in ECD's "regenerative fuel cell" program.

An ECD regenerative fuel cell?

Except for perhaps a handful of people, nobody seems to have heard of that one, a fact acknowledged by ECD president Stanford R. Ovshinsky in a telephone interview. Ovshinsky told *H&FCL* that ECD sent "some information" about it to a few experts for evaluation, apparently in the context of the Texaco buy-in. But ECD has released nothing publicly about the device, he added, "and we don't intend to," at least not for now.

### No Platinum, Proprietary Catalyst

An Ovonic Hydrogen Technology viewgraf sent to *H&FCL* said the device's special features include "proprietary Ovonic non-noble metal catalyst, Ovonic proprietary production technology, high operating voltage, high efficiency, capability to handle regenerative braking energy, wide operating temperature range (-20° - 120°), instant start capability."

Ovshinsky said the term "regenerative fuel cell" may be somewhat misleading. To most fuel cell people it means the ability to electrolyze water as well as producing electricity from hydrogen and oxygen, but "that's not what we are talking about. We do that, but that's just an ordinary part of its function. We have something more serious in mind. We may have to change that (term)."

What it does mean is that the ECD device, whatever its name, has the ability to recover regenerative braking energy, he explained, and to convert it into some form of storable energy.

A second aspect of the device has to do with cost, specifically with the use of noble metals. "We don't use platinum or anything like that," Ovshinsky said, replacing it with a variant of Ovonic's amorphous or disordered materials. "We have a special catalyst," apparently developed for other purposes, that "would do away with the cost and the poisoning," he added. It also seems to play a role in the instant-start capability that ECD claims.

"Even though we are in the battery business, we are also in the hydrogen business," Ovshinksy added. "It doesn't matter to us whether the hydrogen goes as an ion back and forth and charges as electricity or whether you use the hydrogen as a fuel. The fact of the matter is that it's valuable."

### Texaco: Hydrogen is Inevitable

As to Texaco, the company sees its 20% stake in ECD as a down payment into a cleaner energy future. "While oil and gas will remain the dominant energy resource for the foreseeable future, hydrogen will inevitably become part of the energy mix," said Texaco senior vice president William M. Wicker at the early-May press conference when the deal was announced. "Texaco aims to be a leader in the development and commercialization of environmentally smart alternative energy technologies."

And ECD was obviously happy with having found such a potent partner: "We are very pleased with this alliance which will move our advanced technology into the marketplace and be an enabler for the coming hydrogen economy," Ovshinky and ECD chairman Robert C. Stempel said in a joint statement. *Contacts: Texaco, Inc., Paul Weeditz, 713/752-6475; ECD, Stanford R. Ovshinky, 248/280-1900.*

## Hyundai Signs Deal with IFC to Build Demonstrator $H_2$ Fuel Cell SUVs

Reprinted with permission from the *Hydrogen and Fuel Cell Letter*, June 2000.

*Ann Arbor, MI*—Korea's Hyundai Motor Co. and International Fuel Cells have signed an agreement to build at least two and perhaps four fuel cell-powered demonstrator sports utility vehicles, Hyundai said in a late May announcement.

The companies agreed to install IFC's 75 kW single-stack fuel cell, which operates at near ambient pressures in Hyundai's brand new Santa Fe SUV that will be launched in the U.S. this summer in its commercial 4-cylinder and V-6 versions.

Hyundai expects to unveil the "preliminary results of this collaborative agreement," as the release put it, by the end of first quarter of next year. The company didn't say so, but it apparently means that Hyundai

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hopes to show off its new star car at the 2001 Detroit Auto Show—if all goes well with the conversion and there are no unforeseen glitches.

The IFC fuel cell will use hydrogen. The release didn't say anything about how the hydrogen would be stored on board. Efforts to reach executives from either company to get more details failed because just about all the key people involved in the project were reportedly en route between Korea and the United States.

**Electrical Systems by Enova**

The Santa Fe will use a conventional car battery for startup. Enova Systems, of Torrance, CA, a major developer of electric and hybrid drive trains for Hyundai, will supply the electric drive train and power management systems.

"We expect performance of the fuel cell Santa Fe to exceed that of our standard model, making this Santa Fe the first high-performance fuel cell SUV," Dr. Young-Woo Kim, president of the Hyundai America Technical Center, here said in the release. "By eliminating the high-pressure requirements of other fuel cells, IFC has created a system that is much simpler," Kim added. "Eventually, that will translate into lower costs for the consumer."

"We are pleased to be participating in such an innovative development effort as we continue our program of creating fuel cells for transportation and residential use," added IFC president William T. Miller.

One industry insider called the arrangement a "perfect fit" for IFC. Hyundai is "very aggressive," said one. "They want to move as rapidly as possible, and fuel cells have a much higher priority for this relatively small company than for some of the big ones. They are very excited about this. They didn't want the agreement just for the p.r. value, but they will be pushing real development."

Further down the road, Hyundai "may be looking at fuel cell buses as well," according to this source. *Contacts: Hyundai (media), Chris Hosford, 714/965-2370; IFC (media), Michael London, 203/261-1549.*

POWER WINGS

A British patent filed by Brazilian Mario Cavalheiro outlines a technique for electricity generation using fluid raised by pendulum power. The device comprises a cylinder which fills and empties water or other fluid by the movement of a wing-shaped pendulum.

Check out Patent GB 2 341 209 for further info and diagram. (www.esp@cenet.com)

❖ ❖ ❖

The British government is backing energy crops and farmers are being encouraged to grow plants such as coppis for use in electricity power generating stations.

❖ ❖ ❖

A NECESSARY INVENTION

Trevor Baylis, inventor of the clockwork radio and self-styled champion of undervalued inventors, is to become the UK government's "inventions envoy."

He was appalled to discover that the daughter of a friend had the board game she had designed for a school examination stolen by an unscrupulous games designer. He intends to alert schoolteachers to the dangers by insisting they do a mandatory course on intellectual property.

*—Soo Seddon*

"With over 50 foreign cars already on sale here, the Japanese auto industry isn't likely to carve out a big slice of the U.S. market for itself." —*Business Week*, 1968

Submitted by Marcello Truzzi

**Clean Electricity Is Now Just a Click Away**

by John Coviello

Utilizing solar, wind, or hydro generated electricity (a.k.a. renewable energy) has traditionally meant a large commitment both financially and personally by an environmentally concerned homeowner. But renewable energy will soon only be a click away if an emerging force in the renewable energy community called GreenMountain.com can successfully implement their business plan throughout the United States.

GreenMountain.com has been offering their renewable energy products to citizens of California and Pennsylvania for more than a year, and recently entered the New Jersey residential electricity market. So far they've signed up over 100,000 customers in Pennsylvania and California combined.

Here's how it works. A homeowner who lives in a state in which GreenMountain.com offers their renewable energy electric service signs up for their service at http://www.greenmountain.com/. (New Jersey requires a wet signature contract to switch electrical suppliers, so GreenMountain.com mails a contract to people who sign up from New Jersey and the customer mails the signed contract back.) Once a customer has signed up properly, GreenMountain.com informs the customer's local electric utility that the customer is switching to GreenMountain.com. The local utility then informs the customer when their electrical service will be switched to GreenMountain.com. When the effective date arrives, the customer is seamlessly switched to GreenMountain.com. GreenMountain.com has a contract with the local utility to provide clean solar, wind, hydro, and biomass electricity to the utility's grid. The specific amount of clean electricity that GreenMountain.com delivers to the utility's grid depends upon the amount of renewable energy used by GreenMountain.com's customers in a calendar year. The local utility remains responsible for maintaining the electric lines servicing the customer's house.

The renewable energy products that GreenMountain.com offers vary in the three states that it currently serves. They range from modestly renewable products to exclusively renewable products. The modestly renewable products typically cost the consumer about 3% to 8% more than conventional electricity, while the exclusively renewable products typically cost the consumer 10% to 20% more than conventional electricity. For example, a GreenMountain.com customer who lives within the PSE&G utility territory in New Jersey and typically receives a monthly electricity bill from PSE&G for $100.00 would pay $103.09 (3% more) for GreenMountain.com's modestly renewable product or $110.68 (11% more) for GreenMountain.com's exclusively renewable product.

Consumers who choose GreenMountain.com as their electricity supplier support the development of new renewable energy resources. GreenMountain.com plans to invest between 1% and 25% of their monthly revenues into new renewable energy projects. The exact amount that they invest in new renewable energy projects is determined by which renewable energy products their customers choose. For example, GreenMountain.com is currently building a 10 megawatt wind farm in western Pennsylvania to meet the demands of its growing customer base in Pennsylvania. The new wind farm is expected to be operational by early summer 2000. You can even watch them build it via the GreenMountain.com website.

The beauty of GreenMountain.com's approach is that it opens up renewable energy to a whole new segment of the electricity consuming population. Now apartment dwellers, condo owners, and house renters can support the growing renewable energy industry by purchasing their power from GreenMountain.com. Even environmentally aware homeowners who do not have the money or initiative to install a renewable energy system on their house can participate in the renewable energy revolution by signing up with GreenMountain.com. GreenMountain.com's clean electricity is only a click away for those who are willing to pay a little more per month for renewable energy. GreenMountain.com's renewable energy products should be available in other states in the coming years, as more states deregulate their electricity markets.

*John Coviello is a freelance writer who lives in New Jersey. He can be reached at johnwc@rcn.com.*

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## A Letter from *Infinite Energy* to the Next Director of the Office of Science at the U.S. Department of Energy

Professor Mildred Dresselhaus of MIT, in the Department of Electrical Engineering and Computer Science, is a National Medal of Science winner and a past President of both the American Physical Society (APS) and the American Association for the Advancement of Science (AAAS). She has had a continuing peripheral interest in cold fusion, through her contacts with cold fusion investigators MIT Professors Peter Hagelstein and Louis Smullin in her Department. Professor Dresselhaus' current research concerns carbon nanotubes; she is also working to complete a book on that subject.

Last November, Professor Dresselhaus was asked by Energy Secretary Bill Richardson to accept the position of Director of the Office of Science at the U.S. Department of Energy. It is noteworthy that MIT Institute Professor John Deutch held the same position from 1977-79 in the Carter administration. Her confirmation by the U.S. Senate is a foregone conclusion. After learning of her appointment, I sent this letter to her:

Photo, IE Archives

Prof. Dresselhaus

May 18, 2000
Prof. Mildred S. Dresselhaus
MIT Room 13-3005
77 Massachusetts Avenue
Cambridge, MA 02139-4307

Dear Prof. Dresselhaus:

Congratulations on being appointed by President Clinton to be the nominee for Director of the Office of Science at the Department of Energy. This is a very encouraging development, because I know that you have kept an open mind on the subject of cold fusion, low-energy nuclear reactions, despite your signature on the 1989 ERAB Cold Fusion Panel report.

We welcomed your recent attendance at our cold fusion session at the APS meeting in Minneapolis on March 20. I trust that MIT Professor Peter Hagelstein's theory presentation plus the experimental results presented by Dr. Edmund Storms, Dr. Michael McKubre, and Dr. Thomas Claytor (whose paper was read by Dr. Storms) may have impressed you.

In my view, a very careful re-assessment of the cold fusion matter is long, long overdue at DOE. I am sure that I speak for many of our supporters and readers as I make this heartfelt plea to you to launch a scientific re-examination of the issue within the DOE. Frankly, I do not understand how DOE will be able to resist such reconsideration much longer. Since you are a member of the National Academy of Sciences, perhaps that would also be a useful venue for a re-consideration of the evidence and prototype technologies that have emerged.

To provide you with additional encouragement, I have enclosed our video documentary, "Cold Fusion: Fire from Water," which I hope you will have a chance to view. My book, *Fire from Ice: Searching from the Truth Behind the Cold Fusion Furor* (John Wiley & Sons, 1991) is certainly known to you and is in the MIT library system. A new book, by yet another MIT graduate, Charles Beaudette (S.B, EE, 1952), has just come out (*Excess Heat: Why Cold Fusion Research Prevailed*, May 2000) and we are distributing it. I have enclosed a complimentary copy.

I have also enclosed my memorandum/ essay to President Clinton ("The Strange Birth of the Water Fuel Age: The Cold Fusion 'Miracle' Was No Mistake") that was requested (via a telephone call to me in February by the White House Office of Communications). This essay will be read by the President, I am assured—part of a group of a few dozen such essays by others on futuristic technologies and policies. Our colleague and supporter, Sir Arthur C. Clarke, facilitated this memo to the President.

I have also enclosed several pertinent back issues of *Infinite Energy*: No. 24, which celebrated the tenth anniversary of the cold fusion discovery (you may have seen this already); No. 29, which has a rather good article on some shortcomings of E-M theory, courtesy of ex-MIT Professor Parry Moon, and the latest issue, No. 31, which has an excellent article by Dr. Storms on calorimetry issues in cold fusion experiments.

I look forward to hearing your views, and if I do not hear from you before you begin, good luck in your new and challenging position.

Sincerely,
Dr. Eugene F. Mallove, Director
New Energy Research Laboratory (NERL)
Editor-in-Chief, *Infinite Energy* Magazine

## BlackLight Power Corp. Files Lawsuit Against the U.S. Commissioner of Patents

In March, prominent new energy company BlackLight Power, Inc. of Cranbury, New Jersey filed a federal lawsuit against Q. Todd Dickinson, Commissioner of Patents at the United States Patent and Trademark Office (USPTO). The suit was prompted by the USPTO's actions against BlackLight Power's emerging patent position, which covers revolutionary hydrino (sub-ground state hydrogen) compounds and energy production. The suit for "declaratory and preliminary and permanent injunctive relief" (Case Number 1:00CV00422) was filed in United States District Court for the District of Columbia.

The Complaint (suit) lists the Board of Directors of BlackLight Power as parties to the suit: Dr. Shelby T. Brewer (M.S. and Ph.D degrees from MIT in Nuclear Engineering, former Assistant Secretary of the Department of Energy and former CEO of Combustion Engineering's Nuclear Business); George A. Sawyer (former Assistant Secretary of Navy); David Blake (presently with Conectiv—a multi-billion dollar electric utility that has invested in BlackLight and formerly a top manager with DuPont and Hercules Chemical Specialities Co.); and Michael P. Kalleres (former President and CEO of global Associates, Ltd., Technology Services Group, and retired Vice Admiral from the U.S. Navy, currently serving on the Defense Science Board, the Naval Studies Board of the National Academy of Science, and the Dean's Advisory Council of Purdue University). Also noted in the suit is that many of Blacklight Power's shareholders are Ph.D. chemists and physicists.

The Complaint claims as facts matters of great interest to those interested in

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new energy sources:

• That since 1991, BlackLight Power has spent over $10 million in its hydrino energy and hydrino hydride compound research. Well-known companies that have invested include AMP Corporation, Conectiv, and PacificCorp.

• "The heat from the [hydrino] process can be converted to useful energy, for example by building pressure to turn a turbine for a generator or an engine. The new compositions of matter can be used to provide better batteries the size of a briefcase that would drive an automobile 1000 miles at highway speeds on a single charge, without the use of fossil fuels. The new compositions of matter include conductive, magnetic plastics that will revolutionize circuitry and aerospace engineering. The new compositions of matter also include very high strength coatings which are resistant to corrosion."

• That over twenty-five well-known independent laboratories have demonstrated that "the technology is based on sound scientific principles."

• Dr. Johannes Conrads, retired Director and Chairman of the Board of the Institute for Low Temperature Plasma Physics at Ernst Moritz University in Greifswald, Germany "tested the Mills technology and reported to the ACS [American Chemical Society] meeting in October 1999 that it produced an energetic plasma in hydrogen with no power input, conclusively demonstrating a new source of chemical energy from hydrogen."

• The USPTO informed Dr. Mills that patent application 09/009,294 (the '294 application), entitled "Hydride Compounds," *was due to issue as U.S. Patent No. 6,030,601* (the '601 patent) on February 29, 2000. (Regular *Infinite Energy* readers may recall that Mills and BlackLight were earlier, February 15, 2000, awarded another extensive patent, 6,024,935, "Lower-Energy Hydrogen Methods and Structures." See *IE*, No. 30, p. 4)

• On February 17, 2000, the USPTO issued a notice that: At the request of the Director, Special Program Law Office, the USPTO was withdrawing the '601 patent from issue "to permit reopening of prosecution." The notice of this action is attached as Exhibit 1 of the present Complaint.

• The issuing official, Frances Hicks, Petitions Examiner, Officer of Petitions, Office of the Deputy Assistant Commissioner of Patent Policy and Projects, told BlackLight counsel Jeffrey S. Melcher, Esq. (of Farkas & Manelli, PLLC, Washington, D.C.) *that she did not know why the petition to withdraw had been issued*. In the words of the Complaint: "Counsel was subsequently told by Ms. Hicks that the petition had been issued at the demand of Robert Spar, the Director of Special programs, who believed, based on the publicity arising from the issuance of the '935 patent, that the technology disclosed in that patent was not patentable as it was based on 'unproven science' related to cold fusion."

• BlackLight Power was subsequently told by Esther Kepplinger, Director of Group 1700 (the group responsible for processing Dr. Mills' applications), that the petition to withdraw was issued at the request of Commisioner Dickinson.

• Commissioner Dickinson has refused to reverse the Patent Office actions.

The upshot of the BlackLight contentions is that four other allowed BlackLight patent applications, the issued patent '935, plus the withdrawn patent '601, will be opened for re-examination by the USPTO. BlackLight further contends that none of the USPTO personnel involved in the decision to withdraw the '601 patent from issuance had "independently reviewed the prosecution file." In a final statement of facts that highlight the significance of this lawsuit, BlackLight asserts:

"Based on issuance of the '935 patent, the proposed issuance of the '601 patent, and the proposed issuance of the other Four Allowed Applications, BlackLight Power had taken steps for going public and had limited its choice of underwriter to Morgan Stanley or Goldman Sachs. The filing statement for the initial public offering is scheduled for late March 2000. Based on current market conditions, the market capitalization is expected to exceed one billion dollars."

The remainder of the material in the complaint seeks immediate court-ordered injunctive relief from the USPTO's actions, based on allegations that the actions of the USPTO are against statute, are arbitrary and capricious, and run against the USPTO's own regulations. BlackLight Power charges that the USPTO actions will "delay and otherwise jeopardize the scheduled initial public offering and delay commercialization of the Mills technology. This will cause significant financial damage to Blacklight Power that is impossible to quantify. . .Unless restrained, therefore, the Patent Office's and Commissioner Dickinson's actions will irreparably harm BlackLight Power."

The key "Request or Relief" portion of the Complaint reads:

> WHEREFORE, Plaintiff BlackLight Power Inc. respectfully requests that judgement be entered against the Patent Office for the following relief:
>
> A. A temporary restraining order and preliminary and permanent injunctive relief restraining the Patent Office from withdrawing the '601 patent from issuance and directing that the patent be issued.
>
> B. Preliminary and permanent injunctive relief restraining the Patent Office from withdrawing the Other Four Allowed Applications from issuance and directing that these issue as patents.
>
> C. A declaration that the withdrawal of the '601 patent from issuance is arbitrary and capricious and contrary to law, in violation of the Administrative Procedure Act.
>
> D. A declaration that the proposed withdrawal of the other Four Allowed Applications from issuance is arbitrary and capricious and contrary to law, in violation of the Administrative Procedure Act.
>
> E. Any other just and equitable relief the Court deems appropriate.

Attachment 2 to the Complaint is a letter from Atty. Melcher to Esther Kepplinger of the USPTO. The letter notes: "BlackLight Power's current private market capitalization is already in excess of 340 million dollars, based on the last private placement that was oversubscribed." Melcher also states: "These positive tests [of Mills technology] have not escaped the attention of the U.S. Government. Following a thorough review, the United States Navy has taken affirmative steps to develop applications based on the Mills technology in cooperation with BlackLight Power." Melcher states that Kepplinger informed him that the '601 application "had been 'pulled' based on some perceived 'heat'(from an undisclosed source) without reviewing the file history." Melcher strenuously objects to the USPTO's attempt to introduce the issue of "cold fusion" into its actions against the Mills' applications, when in fact Mills has made no claims of nuclear changes in the hydrogen-shrinkage reactions and processes that his patent applications specify, and when the examiners who had actually dealt with the applications had already agreed to dissociate cold fusion claims from the Mills claims.

There is more news about BlackLight Power. Journalist Erik Baard continues to write about the company for *The Village Voice* and other publications (see *IE*, No. 29, p. 40 and *IE*, No. 31, p. 38). The space exploration oriented website, space.com (founded by ex-CNN business news anchor Lou Dobbs), ran several stories by Baard on May 23. The articles note that NASA space station engineer Luke Setzer has founded an internet discussion group ("Hydrino Study Group") on which active, moderated discussion of the Mills theories and technologies

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appear. (To subscribe, e-mail to hydrino-subscribe@eGroups.com.) Dr. Mills himself has been participating in the technical discussions. One of Baard's articles quotes Nobel laureate Steven Weinberg that he would "bet his life" that Mills is wrong and, "As far as we know, quantum theory is rigorously valid. I have no idea what would replace it." Baard broke the news that National Radio Astronomy Observatory astronomer Barry Turner, while respecting Mills' laboratory work but having significant reservations about the Mills theory, is proposing to focus radio telescope observations on regions of space that might confirm Mills' prediction that emanations from interstellar hydrinos constitute a pervading interstellar "dark matter." This would require the use of a new radio telescope still under construction at the Greenbank, West Virginia facility in the 1 mm and 1 cm wavelength regions. He quotes Turner: "There are some people who will say that 'Dr. Turner must have lost his marbles. . .' I owe it to science. I think nature would be fascinating if he's right."

Baard, who visited the Mills laboratory and witnessed high temperature hydrogen plasma experiments in progress that seem to reveal the power of the claimed hydrino reactions, also reports that significant technological progress is being made by BlackLight. Engineering consulting firm NovaTech is working with BlackLight to develop an electricity-producing device—a "gyrotron," which could be operating within the next six months. Also very telling is this description from Baard: "Spectral Data Services, the largest contract nuclear magnetic resonance laboratory in the U.S., has tested hydrino hydride compounds. Company President Gary Turner, a physical chemist, confirmed that hydrino compounds 'register a strange chemical shift. I'd have to rank it up there in the top-ten weird observations I've made in my career. It's something new and different, and well, that happens.'"

### The Mystery of Eugene Island 330*
### By Christopher Cooper

Eugene Island is a submerged mountain in the Gulf of Mexico about 80 miles off the Louisiana coast. The landscape of Eugene Island is riven with deep fissures and faults from which spew spontaneous belches of gas and oil. Up on the surface, a platform designated Eugene Island 330 began producing about 15,000 barrels of oil per day in the early 1970s. By 1989, the flow had dwindled to 4,000 barrels per day. Then, suddenly, production zoomed to 13,000 barrels. In addition, estimated reserves rocketed from 60 to 400 million barrels. Even more anomalous is the discovery that the geological age of today's oil is quite different from that recovered 10 years ago. What's going on under the Gulf of Mexico?

It is difficult to avoid the conclusion that the oil reservoir at Eugene Island is rapidly refilling itself from "some continuous source miles below the earth's surface." In support of this surmise, analysis of seismic records revealed a deep fault which "was gushing oil like a garden hose."

The deep-seated oil source at Eugene Island strongly supports T. Gold's theory about The Deep Hot Biosphere. Gold holds:

> . . .that oil is actually a renewable, primordial syrup continually manufactured by the earth under ultrahot conditions and tremendous pressures. As this substance migrates toward the surface, it is attacked by bacteria, making it appear to have an organic origin dating back to the dinosaurs.

The apparent deep-seated oil source at Eugene Island and Gold's ideas make petroleum engineers wonder about a similar situation at the seemingly inexhaustible oil fields of the Middle East.

> The Middle East has more than doubled its reserves in the past twenty years, despite half a century of intense exploitation and relatively few new discoveries. It would take a pretty big pile of dead dinosaurs and prehistoric plants to account for the estimated 660 billion barrels of oil in the region, notes Norman Hyne, a professor at the University of Tulsa in Oklahoma. "Off-the–wall theories often turn out to be right," he says.

*Originally appeared as "It's No Crude Joke: This Oil Field Grows Even as It's Tapped," *Wall Street Journal*, April 16, 1999. Cr. C. Casale.

Reprinted with permission from *Science Frontiers*, 3, p. 3. (Available from The Sourcebook Project, PO Box 107, Glen Arm, MD 21057.)

### Park's *Voodoo Science* Making the Rounds

Despite its grotesque caricature of cold fusion as "fraud," Robert Park's new book, *Voodoo Science: The Road from Foolishness to Fraud*, has gathered wide

*Briefs continued on page 66*


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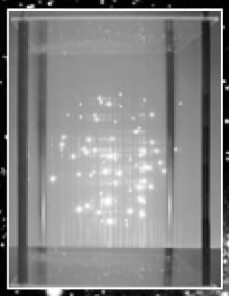

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# Book Reviews

**Excess Heat: Why Cold Fusion Research Prevailed**
by Charles G. Beaudette
ISBN 0-9678548-1-4
$26.95 Paperback, 365 pp.
Oak Grove Press, 2000

**Excess Heat is available from Cold Fusion Technology, Inc. See ad on page 5.**

**Review by Jed Rothwell**

This book is a good introduction to some of the major issues in mainstream, palladium-based cold fusion, especially calorimetry, excess heat, and the early history of the field. Most of the book is about the controversy, and philosophical issues such as the different ways the term "replication" is used in biology, chemistry, and physics. Beaudette, a graduate of MIT with a degree in electrical engineering, explores the roles of experiment versus theory and explains why arguments which cannot be falsified have no meaning. He compares cold fusion to other controversial discoveries and discoveries which were difficult to replicate, such as the Wasserman test for syphilis and the cloning of adult sheep. He discusses discoveries which could not be explained for many years, such as superconductivity and heat from radioactivity. Most of the technical discussions in the book are about heat, with short sections covering other evidence of nuclear reactions such as tritium, helium, and transmutations. Beaudette summarizes his main theme:

> The scientific community failed to treat the Fleischmann and Pons phenomenon as a scientific observation. Protocol required evaluation of the observed outcome as a stand-alone claim. Instead, the community demanded a theoretical underpinning, nuclear products, replication on demand, and understanding. These demands constituted an egregious distortion of historical procedure.
>
> To discard a well-made observation is to violate modern protocol. . . The most interesting and perplexing observations, though accurately measured, would have to be refused by the scientific community because their cause was obscure.
>
> Does this mean that any claim of observation must be accepted as worthy of scientific study? Certainly not. It means something quite different. It means that the controversy must center about the quality of the measurements and not about the source or cause of the phenomenon. (p. 173)

The text is loaded with specifics and quotes. Thirty-three graphs and figures illustrate excess heat, tritium levels, the statistical concept of a critical threshold, and other topics.

Beaudette agrees with Fleischmann's dictum, that heat is the principal signature of the reaction. When Eugene Mallove first saw the manuscript, he thought that Beaudette was downplaying the importance of tritium and other nuclear evidence, or trying to compartmentalize the field, separating the overwhelming and clear evidence of heat out from sporadic tritium, and the still-fuzzy, mostly unconfirmed reports of transmutations. The skeptics sometimes use this "divide and conquer" strategy, calling into question first heat, then tritium, without admitting they are mutually supportive evidence of a nuclear reaction. Beaudette's arguments are more subtle. He does compartmentalize inasmuch as he insists heat alone proves the reaction cannot be chemical, and when he insists that the skeptics should evaluate calorimetry on its own merits, without tying it to plasma fusion theories which predict neutrons. Heat plus tritium minus neutrons adds up to a mystery, not a mistake. Beaudette wants to escape the sterile, circular reasoning in the closing sentences of Huizenga's book:

> If the reported intensity of nuclear products is orders of magnitude less than the claimed excess heat, then the excess heat is not due to a nuclear reaction process.
>
> Furthermore, if the claimed excess heat exceeds that possible by other conventional processes (chemical, mechanical, etc.), one must conclude that an error has been made in measuring the excess heat.[1]

Huizenga's logic makes one feel like an ant desperately seeking the exit lane on a Mobius strip. Beaudette says the only way you can conclude an error has been made is to *find* an error.

*Excess Heat* is not for the casual reader. As Nagel says in the introduction, it reads like a legal brief. It is an exhaustive analysis of some narrow aspects of cold fusion, somewhat like EPRI's massive report, *Development of Advanced Concepts for Nuclear Processes in Deuterated Metals*.[2] Beaudette is an electrical engineer and he does what engineers do best: he methodically hammers every point home and bulldozes away objections. He presents concepts in meticulous, quantitative detail, with definitions, examples, counterexamples, and historical and philosophical perspective. He gives the skeptics plenty of rope with which to hang themselves, quoting them extensively, presenting their arguments more skillfully than they do themselves.

Several concepts are repeated in different parts of the book, for different purposes: first in an historical then an analytical context, then with reference to related ideas, and again in the closing arguments. Take the concept of "pathological science," a favorite theme of the skeptics. It is methodically beaten to death. On page 60, we learn that the term originated in a lecture by Irving Langmuir, in 1953. Langmuir listed six criteria and described N-Rays, an overworked example of what he had in mind. Beaudette shows how some of the six criteria did apply to cold fusion at first, but none of them do now. N-Rays do not resemble cold fusion.

Langmuir said that with pathological ideas, the ratio of supporters to critics reaches 50% and then falls to zero. Douglas Morrison interpreted that to mean the ratio of successful to unsuccessful heat experiments falls to zero, and he has been waiting for that to happen ever since. In fact, most experiments performed today succeed. The absolute number of experiments has declined drastically, because cold fusion is out of favor and many cold fusion scientists have retired or died, but of the experiments still performed, most produce positive results.

On page 66, we learn that Robert Park invented a new definition in his book *Voodoo Science*. He said pathological science means anything which "cannot be explained by science."

On page 159 Langmuir is introduced again, and his criteria again summarized, as Beaudette discusses a paper by skeptics Crease and Samios. They agreed that cold fusion does not fit Langmuir's original description, so they make up a new category which they call "degenerate science." They invented four characteristics:

1) Too many miracles were needed.
2) The "discoverers" were outsiders.
3) The "discoverers" have not tried to kill the discovery.
4) Inability to repeat the experiment was met by *ad hoc* excuses.

Lo and behold, they found cold fusion fit this new definition. Except that Beaudette shows it does not fit. No miracles are required after all. Pons and Fleischmann were insiders: they are established experts in electrochemistry and calorimetry. Crease and Samios are the outsiders: nuclear physicists. Researchers did try to "kill" the effect in every sense: they looked carefully for arti-

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facts, performed blank experiments, and found reliable ways to turn off the heat. Regarding point four, Beaudette writes, "science did not require repeatability, replication was sufficient." He explains this subtle distinction in Chapter 6. I would add that when experiments failed, researchers often gave plausible reasons (not "excuses") which would be familiar to any electrochemist, such as surface contamination, cracks which prevented loading, poisons known to inhibit other electrochemical reactions, light water contaminating the heavy water, and so on. Researchers have shown that when you correct these problems, the cold fusion reaction occurs, and when you deliberately cause them, by adding light water to a cell for example, the reaction goes away.

Robert Park pops up again with yet another definition. In a lecture at the APS in 1995 he said beware of pathological science. "It is a thin line between foolishness and fraud . . . most screwy sounding scientific claims—are screwy." The problem is, we still do not know what to watch out for. Any new ideas may "sound screwy," whatever that means, just because they are novel. Superconductivity and the excess heat from radium must have looked screwy at the turn of the twentieth century. Beaudette says, "In order to apply his list [of pathology symptoms], one must first identify the science as either foolish, or screwy, or charlatanic. If that can be done, however, the lessons themselves are not needed." Park's advice boils down to: "Don't make a mistake," which is about as useful as the stock market tip: "Buy low, sell high."

On page 271 we learn how the editor of *Scientific American*, Jonathan Piel, invented yet another new definition which would make Langmuir spin in his grave. He said pathological science means a phenomenon for which "the precise physical mechanism is not fully understood at the moment."

This meandering history proves that "pathological science" means whatever a skeptic feels it should mean, or whatever definition pops into his head. I am glad someone went to the trouble to document some of the strange ways this concept has been defined and redefined. Historians will appreciate Beaudette's careful investigation. But does it merit this much attention? In *Fire from Ice* Mallove described "pathological science" in a few pages and concluded it is at best an "interesting parlor game" but "definitely not science." He dismisses it and moves on.[3]

Most of the book is devoted to careful analysis of facts, data, and to drawing careful conclusions from the data. Some is devoted to the human story. An interview with Bockris is related. Pons' family background is described. The skeptic's misbehavior is described vividly. Reading about it is depressing, excruciating, and at times infuriating.

A heartbreaking chapter titled "Un Cri du Coeur" describes the experiences of Prof. David Goodstein from CalTech. He has observed cold fusion experiments and found no mistakes in them, yet it seems he cannot bring himself to act on his own beliefs, training, and observations.

Beaudette thinks the early history of cold fusion soured the field and shaped events. He regrets that cold fusion got off on the wrong foot with the University of Utah press conference. He thinks the name "cold fusion" is a misnomer which has confused the issue. Storms and others have coined new names like LENR partly to escape from the stigma of the original one. A new name would be a euphemism. The stigma associated with the original word will soon attach to the new one. The 1989 introduction could have been done with more finesse, but I doubt it would have made much difference. Beaudette knows that some level of controversy was unavoidable: "Revolutions . . . always hit hard and they hurt. The notion that somehow—if only things were handled better—the deep divisions could have been avoided is not a realistic sentiment."

After dissecting skeptical arguments for several chapters, Beaudette concludes that the skeptics play no useful role in this debate. "Commentary by the skeptics cannot be expected to contribute to solving outstanding questions because the skeptics do not acknowledge these questions exist."

The book may be a little difficult for a nontechnical reader in places, although Beaudette carefully defines and illustrates concepts such as threshold value, recombination, and calibration. The most involved technical discussion, regarding the Wilson paper,[4] is pushed into an Appendix. Here is the typical level of detail: "When you are looking for experimental validation, it is essential that a common fault not reappear in different laboratories. It is of special importance that Oriani used a Seebeck-effect calorimeter. The Seebeck design surrounds the cell with more than a thousand thermocouples (tiny metallic devices that respond to temperature differences) connected electrically in series . . ." (p. 192) Beaudette likes to cram a lot of information into each paragraph. The first two sentences might be easier to understand with a little expansion: Instruments based on different physical principles cannot suffer from the same systematic errors. You can be sure a temperature is correctly measured when you use a thermocouple and a mercury thermometer and they give the same answer. A person who has done experiments will know this, but others may have to read these lines a few times to get the idea.

Although this book describes a depressing history, it is not heavy handed or preachy. It is enlivened by Beaudette's wit and serene common sense, and by his faith in the scientific method. Beaudette concludes that cold fusion "prevailed," as he says in the title. He sees slow but sure progress solving the technical problems, and he thinks the system will ultimately work, and cold fusion will be given a fair chance. I hope he is right.

### References


**1.** Quoted by Beaudette, p. 262, from: Huizenga, John R. 1993. *Cold Fusion: Scientific Fiasco of the Century*, New York: Oxford University Press, 285.

**2.** McKubre, M.C.H. *et al.* 1994. "Development of Advanced Concepts for Nuclear Processes in Deuterated Metals," TR-104195, Research Project 3170-01, Final Report, August.

**3.** Mallove, E. 1991. *Fire from Ice: Searching for the Truth Behind the Cold Fusion Furor*, New York: John Wiley & Sons, 280-282.

**4.** Wilson, R.H. *et al.* 1992. "Analysis of Experiments on the Calorimetry of LiOD-$D_2O$ Electrochemical Cells," *Journal of Electroanalytical Chemistry*, Vol. 332.


## The Giza Power Plant: Technologies of Ancient Egypt

by Christopher Dunn

ISBN 1-879-18150-9

$18.00 Paperback, 281 pp.

Bear & Co., 1998

**Review by Mike Carrell**

Dunn's book has two distinct parts. The first deals with well-formed inferences that can be drawn from the very stones of the Great Pyramid at Giza as seen by a master craftsman. The second is a series of speculations about the structure of the Great Pyramid as an energy generator.

In Egypt the Giza plateau stands as an enigma which has confounded several generations of investigators who have attempted explanations of its purpose as well as how the structures were built, by whom, when, and why. The conventional explanation of the pyramids as tombs built by hordes of

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laborers by a primitive society fails on close examination. Dunn's book is one of many by different authors, each attempting a different reading of the site. Dunn's findings are well-summarized at http://www.sunship.com/egypt /articles/cdunn.html, including photographs, and at his website http://www.gizapower.com.

**The Craftsman's View**

I can do no better than quote from Dunn's introduction:

> As a craftsman and engineer, I have worked with machines for over thirty-five years. I began to apply my specialized knowledge to the data gathered about the Great Pyramid. For instance, scholars have suggested that the pyramids were built with primitive hand tools. This is a subject I know something about. I once apprenticed in England, where I worked many hours using nothing but hand tools. Saws, drills, files, and chisels were all we were allowed to use to create precision objects. At the time, I failed to see the benefit of this toil. Why work eight hours a day bent over a piece of steel clamped in a vise when there was machinery in the area that would do the work more quickly and accurately?
>
> The result of this labor was several precision artifacts and—more important—the knowledge and appreciation of what it takes to produce something by hand. It also served to forge continuity between the craftspeople of the Old World and those of the New World. As I evaluated the opinions of Egyptologists about ancient building and machining techniques, my training told me that their theories were lacking at best—and outright wrong at worst. As I looked at the data, in fact, I began to suspect that the ancient Egyptians may have used technologically advanced tools.
>
> Bringing applications from my work as a machinist to bear on my speculations about Egypt and its Great Pyramid, this vague suspicion became a firm belief as I pondered for innumerable hours on the methods that might have been used to produce the various artifacts. I was filled with awe and wonder at the techniques used, and at the same time I began to be aware of a greater appreciation for the technology our own society has developed. I also wondered what future archaeologists would say about some of the artifacts we may leave behind.
>
> With the advances in manufacturing technology, my career has been a continuous educational experience that ultimately guided me into the field of laser processing of materials. During this period, I was asked to give a presentation to a local high school on career opportunities in manufacturing. In preparation for my presentation, I cut two identical cartoon characters out of stainless steel on a 5-axes YAG laser. The machine is a computer numerical controlled machine, and each character had fine detailing with a .010-inch kerf (cut width).
>
> Having a fascination for the analytical skills necessary to determine how prehistoric societies manufactured precision artifacts, I presented one of these laser-cut figures to the class and told them that if our civilization were to be destroyed, future archaeologists may be able to determine the manufacturing tools our civilization uses just by studying that object. The surface of the cut, when studied under a microscope, would show signs of a recast layer, indicating the use of heat in its production, and the fine kerf can only be produced by that heat being focused to a very small spot. The laser, I explained to the class, is the only method that is capable of producing the features found in this object. I then described the various disciplines involved in the creation of the laser. It required physicists, optical engineers, mechanical engineers, materials and electrical engineers, and a host of crafts people building equipment to their specifications. There were quite a few careers involved in the creation of this seemingly simple cartoon character.
>
> To envision this laser-cut object, think of a talented artist drawing free hand with a pencil. The lines where the loser has cut through the material are as thick as a pencil line. Using the law of Occam's razor, where the simplest solution is probably the correct one, it could be assumed that a talented artist created this stainless steel character by guiding a handheld laser. I then produced the other cartoon character and placed them together so that each feature overlaid the other perfectly. Now, I told the class, because the human hand and eye are incapable of producing two objects that have complex features with such precision, the scope broadens. There were obviously other disciplines and careers that had a hand in the cutout. There had to have been some mechanical device to guide the laser along its path. There had to have been controls to turn the laser off—as it passed over the metal—and on again to punch a hole through the metal and begin once again cutting the intricate lines. We need electronics engineers, machine tool designers and builders, and computer engineers and programmers, I explained, to build the computers and write the codes that guide the machine tool and control the laser. Besides introducing the class to the hidden work opportunities that are behind the most simple artifacts, my point was to teach the students that a tool is neither created or used in isolation. What I did not tell them was that the same analytical skills and analysis that are readily accepted when applied to modern artifacts can be equally beneficial when analyzing artifacts from ancient times. The difference is that the tools that created modern artifacts are still in existence, while those that created many ancient artifacts are not.

When I was in middle school, a history teacher asked us to take pennies from our pockets and write down all we might surmise about the civilization that made them.

Dunn goes on in great and balanced detail to survey various proposals concerning the construction of the Great Pyramid, showing where they fail to accommodate established facts and observations. Outstanding among these is the precision with which the whole edifice is built. Shafts leading to the King's chamber, for example, are quite straight, with smoothly aligned segments. These are cooperating surfaces, cut from separate blocks of limestone fitted in the developing structure. The measurements and cuts were done with exact knowledge and measurement, not "carved in place." Quoting again,

> Although the exact precision demonstrated in the manufacture and assembly of the Great Pyramid may have had little significance a century ago, there are, at this time, many people who are intimately familiar with these dimensional tolerances. I am one of them, for many years creating products with tolerances much finer than .010 inch. I know what it takes to hold such fine tolerances—and there is a great difference between knowing what .010 inch is from an abstract academic viewpoint and understanding what .010 inch is from hands-on, practical experience.
>
> This is why I laugh when I hear intelligent men and women proposing that the pyramids and other artifacts were created using hammers and chisels. Other machinists, toolmakers, and engineers with whom I have discussed this issue are equally amused, and normally just shake their heads and mutter something straightforward and unprintable. These workers, the members of what we consider a highly advanced civilization, understand the following: It is very well to dream, speculate, and theorize, but when it comes to doing the work, we are generally brought down to earth and hard facts. The most efficient and economically minded designers and engineers are those who have experienced the manufacturing phase of their ideas and have worked on the bench and with the machines. These experiences lead them to be more realistic in their demands of skilled craftspeople.
>
> Through my own experience in manufacturing, I have realized that theories and ideas that seemed to work fine in my mind, or on paper, could be rendered unworkable when I actually tried to apply them. In much the same way, I have found that many theories regarding the building of the Great Pyramid are not supported with material proof, for no one, despite numerous attempts, has been able to duplicate the structure using the methods theorized to have been in place in ancient Egypt. These methods have been applied, with limited success, in building smaller structures, but they are not attempts to replicate the more difficult aspects of the building. A pyramid that is twenty- or fifty-feet tall and built with limestone blocks that weigh no more than two tons does not explain how the ancient pyramid builders raised seventy-ton blocks of granite to a height of two hundred feet. Scaling up a project does not necessarily follow a linear path, nor does it rely solely on a fixed set of assumptions.

Dunn goes on to examine artifacts which show tool marks which strongly suggest that they were made by the equivalent of lathes, milling machines, and drills. (See Figure 17.) A cutting or abrasive tool is moved forcefully and systematically in a controlled

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path to create a complex surface. These are not the work of hand chisels and abrasives. A number of specific artifacts are examined in detail. The matter of the granite boxes illustrates Dunn's point. In a tunnel, twenty-one granite boxes were found, approximately 13 feet long, 7.5 feet wide, and 11 feet high. Dunn placed a precision steel straightedge against the outer and inner surfaces of one of these boxes and shone a flashlight against the far edge; no light showed under the edge, indicating the granite surface was perfectly flat. Never mind how the boxes were moved into the tunnel, how were surfaces so flat produced on the inside as well as outside? It would be a challenging task for modern craftsmen.

The center cavity can be formed in stages. Most of the granite can be removed by repetitive use of cylindrical core drills. A tube, whose lower rim is charged with an abrasive, is rotated and pressed so that a deep cylindrical groove is created. Side pressure can break off a core, which is then removed. The next question: "What was the material of the drill and the abrasives used, and how was the cutting done?" Conventional Egyptology allows bronze, not iron, as the metal and sand as an abrasive. Sand is mostly silica (quartz), which with feldspar and mica makes up granite. Cutting with sand would leave a characteristic finish, which is not seen. Silica would sink into bronze under pressure, making for ineffective cutting.

Sir William Petrie made exhaustive measurements of the pyramids at Giza and was perhaps the first to demonstrate the extreme precision of their construction and alignment. He described a drill core which he found and took home as having a taper and a shallow groove spiraling down its side. This core has become the center of much discussion. If the spiral groove is an artifact of the feed of a cylindrical tube drill, then its feed rate is very high; the only way such a thing can be done with modern techniques is with an ultrasonic drill. *An ultrasonic drill*. Here a magnetostrictive transducer driven at ultrasonic frequencies imparts a repetitive hammering impulse to the abrasive-charged lower edge of the cutting tube.

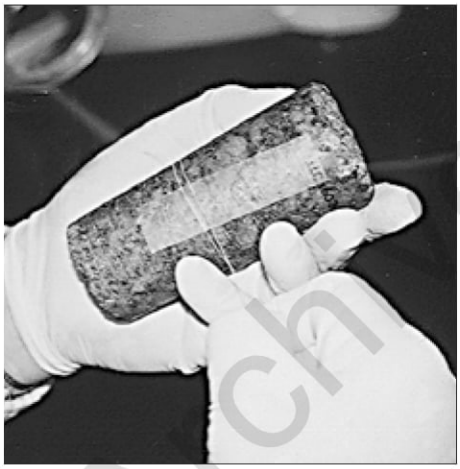

**Photo 1**. The white cotton thread showing the helical nature of the groove. *Photo from www.gizapower.com/petrie/chris6.htm.*

One can concede that iron machines, long ago removed or rusted away, might have been used to produce some of the other surfaces that Dunn describes in his book. Such are within the range of technology attributable to those times from other clues and artifacts that are beyond the scope of this review (the Baghdad Batteries, the Antikytheria Mechanism).

Ultrasonic drilling is altogether something else. It requires knowledge of electronics necessary to produce substantial power at ultrasonic frequencies, knowledge of specialized ferrous alloys that change dimensions in a magnetic field, electrical power sources, etc. There is no other trace of such technology in that time period. It is argued that time of execution is the wild variable here; because twenty-first century industry is hurried does not mean that the Egyptians were also, so more time could be devoted to important projects. This is not a simple matter to be resolved with glib sound bites.

An extensive discussion has arisen about the Petrie core and its interpretation. Recently Dunn visited the Petrie museum in London and inspected the actual core. (See Photo 1.) Photos of the core and Dunn's commentary on his visit are at his website <http://www.gizapower.com>. A collection of other photos of objects and artifacts he discusses are at the sunship.com site cited above. There is also an archive of an extended discussion about ultrasonic machining at both of the websites cited at the beginning of this review.

**Figure 17.** Petrie's samples of machining. A—Alabaster B—Basalt D—Diorite G—Granite L—Limestone. From *Pyramids and Temples of Gizeh*, by William Flinders Petrie.

### The Power Plant

Here I will summarize Dunn's speculative thread about the purpose of the Great Pyramid, which gives the book its title.

It is assumed that the pyramids were built as tombs. There is no evidence that the Great Pyramid was built as a tomb or used as such. What, then, was its purpose? Dunn builds a case that it is a power plant, deriving its energy from oscillations of the earth itself. These are focused by the structure on the King's Chamber, which is made of granite within the larger limestone body of the Great Pyramid. Granite contains quartz, which is known to be piezoelectric (interaction of electric charge and mechanical stress on the crystal). The earth-driven oscillations are at a very low frequency, but where non-linearities exist, harmonics of the driving frequency are also produced whose energy can be concentrated by resonators. Among such resonators are the maser and laser. The former is an acronym for Microwave Amplification by Stimulated Emission of Radiation; the latter acronym is the same for light. The general principle of these devices is that atoms of the lasing gas or crystal can be excited by a general energy source, such as an electric current or discharge. If a resonating body is present, the atoms can be stimulat-

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ed to emit their energies in synchronism to generate a beam of coherent energy, as in the familiar laser.

Hydrogen gas in space emits energy in the microwave range, which enters the King's Chamber by the northern shaft. The King's Chamber is filled by hydrogen gas generated within the pyramid and conducted to the King's Chamber by the Grand Gallery, along with acoustic energy which excites the quartz crystals in the granite to generate the pumping frequency and light through the northern shaft provides the stimulus for emission. The resonator is the King's Chamber and the great coffer a focusing element or amplifier. Above the chamber are five layers of granite beams in which infrasonic vibrations have been detected. The emitted energy emerges through the southern shaft. What would such an energy beam be used for? Dunn speculates that a reflecting satellite might redirect the energy to various parts of Egypt.

**Commentary**

Dunn's construct shows much evidence of painstaking detail and thought.

Where Dunn discusses the techniques necessary to fabricate the extant artifacts, including the pyramids themselves, he is on solid ground and has made an important contribution. Before anyone can claim understanding of the Giza and related sites, all the pieces have to fit. Dunn's work presents a stony obstacle (pun intended) to prevailing views of Egyptology.

Dunn speculates on the technologies that may have been used, with contemporary practices as models. These models, such as ultrasonic machining, imply an extraordinary depth of technology that could be metaphorically said to exist on a higher plateau, unreachable from what else is known about the times. Much the same applies to the power plant hypothesis. Many features of his construct, when carefully examined with a detailed knowledge of contemporary physics, do not behave the way necessary to support the hypothesis. A full exposition of this critique is beyond the scope of this review.

While I am not comfortable with Dunn's hypothesis, neither am I comfortable with any other, and the Giza complex remains as a challenge to our understanding.

Interpretation of the past is a hazardous adventure. Scanty data, unrepeatable experiments, and political and religious agendas all becloud the foggy past. It is so easy to reject inconvenient data. We suffer from temporal chauvinism; our time is the best of all, secure in advanced and correct knowledge. A small problem, the mounting evidence that existing paradigms are threadbare in spots, barely able to conceal the limitations of underlying assumptions.

Writing cuneiform in clay is arduous and lumpy; papyrus can be notably durable but in limited supply, wall paintings and carvings hard to transport. Ancients recorded conquests and names, perhaps some notable philosophy and poetry, but neglect the work of mechanics, artisans, and slaves. Alas, what was lost in the political and religious bonfires in the Library of Alexandria? We have some isolated artifacts that we know must have context, but it is missing in the written records. The "Baghdad Batteries" and the Antikytheria Mechanism both date from the first century BCE.

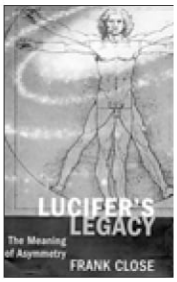
The "Baghdad Batteries" are clearly built by someone who understood what was needed, but the context is gone. With those batteries, one small step could have opened the door to electromagnetism if only someone knew what to do. The Antikytheria Mechanism is a complex geared mechanism with accurate calibrated scales. It is an astronomical analog computer, last set in 87 BCE.

Historians without technical education often misread what is obvious to the technical mind. Dunn has made an important contribution which only deepens the sense of mystery about these monumental structures.

## Lucifer's Legacy: The Meaning of Asymmetry

by Frank Close

ISBN 019-850380-6
$27.50 Hardcover, 256 pp.
Oxford Univ. Press, 2000




**Review by Soo Seddon**

Thanks be to Frank for providing a page-marker gratis with his latest tome.

*Lucifer's Legacy*, inspired by the symmetry of the Tuileries Gardens in Paris, contains a conveniently bookmark sized erratum slip, as the published diagram delineating the source of Prof. Close's inspirational revelation was incorrectly drawn. The schematic was, in fact, asymmetrical, but then the apparent non-uniformity of the universe—nature's erratum slip, if you will—is the entire basis of the book.

For the most part, Close's narrative style is entertainingly informative and it's easy to see how he became the UK media's friendly face of fizzicks—albeit with a less than friendly attack on cold fusion in his 1991 book *Too Hot to Handle*.

It was, he states, a headless statue of Lucifer in the otherwise mirror-perfect gardens of the Tuileries which disturbed the mathematical precision of the layout, creating an unseemly blot on the symmetrical landscape lurking inside the concave entrance walls which curved "like a mother's arms taking her child, welcoming and encouraging one to enter within." It seemed to him like a "metaphor for existence."

The Prof's prolix prose apart, what is his book actually about?

Life, the Universe and nuclear physics, of course. . . The story starts, appropriately, at the very beginning—the post Big Bang battle between newly created matter and antimatter; the unknown imbalance which led to matter's victory and the mystery of the apparently now sparse antimatter; the further disorder as the elements formed with the separation of the EM and weak forces and the mirror asymmetry of organic molecules leading to the conclusion that such differences are essential for the creation of anything "useful."

His quest was to find an answer, and possibly the singular source of asymmetry. A seemingly ambitious project for a guy who good-naturedly confesses his vain attempt to observe the coriolis effect in the washbasin of an airplane making a northern to southern hemisphere flight, only to be thwarted by the notoriously brisk suction power of Boeing bathroom sinks.

Close makes some cute observations on human left/right differences and appears to have an impressive repertoire of suggested party tricks using mirrors and a little exotic role play involving harlequin costumes. Unfortunately, I never get invited to those sort of parties.

I haven't yet managed to completely understand any discussion of quantum physics or bizarre subatomic particles and Close's overview of the subject proved to be no exception to this. A great pity because his initial, comforting "Once upon a time. . ." style lulled me into a false sense of intelligence and I had hoped that he would manage to plug the yawning gap in the market for a coffee table book on nuclear physics that was actually understandable.

Close is very accomplished at relating all that is known and does so in a relatively reader-friendly manner. Of the unknown, however, he states: "Discovery involves being in the right place at the right time, being prepared, and being brave enough to break away from the existing mind-set."

Perhaps his skepticism has mellowed some over the years, or maybe he just overheard some particoloured guest at one of his fancy dress soirees say that. ❑ ❑ ❑

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# New Energy Upcoming Events

## Nuclear Energy in Central Europe 2000

**September 11 - 14, 2000**
**Golf Hotel, Bled, Slovenia**

**Internet: http://kastor.ijs.si/~bled2000 • Email: bled2000@ijs.si**

**Nuclear Society of Slovenia, Bled 2000, P.O. 3000, Ljubljana, 1001 Slovenia**

## Hybrid Vehicles 2000 Conference

**September 11-13, 2000**

**Hilton Windsor Hotel Windsor, Ontario, Canada**

**• Hybrid vehicle program development and outlook from Honda, Ford, Nissan, Volvo, and others • New trends in motors, controllers, power electronics, and energy storage**

**www.intertechusa.com info@intertechusa.com**
**Phone: 207-781-2150 Fax: 207-781-9800**

**Intertech, 19 Northbrook Drive, Portland, ME 04105**

## Texas Renewable Energy Roundup Green Living and Sustainability Fair

**September 29 - October 1**
Fredericksburg, Texas

Entrance to fairgrounds is $5 per person!
www.RenewableEnergyRoundup.com • Russel Smith @ 512-345-5446

## Forum on Converting to a Hydrogen Economy

**September 22-24, 2000**
University Park Holiday Inn
Fort Collins, Colorado

Sponsored by the International Association for New Science
IANS, 1304 South College Avenue, Fort Collins, CO 80524

Phone: 970-482-3731 • Fax: 720-222-2026 • www.hydrogennow.org

## Announcement

### Special Session on LENR/CF at ANS Winter Meeting

You are invited to participate in a special session on LENR/CF at the winter American Nuclear Society meeting, Washington, D.C., November 12-17, 2000. This session is sponsored by the Isotopes and Radiation Division (IRD) and is being organized by George Miley. Paper summaries of 450-900 words, are due by June 15, 2000. For format details and submittal information, see www.ans.org. Submit papers to Mohammad Modarres, Technical Program Chair, ATTN: Transactions Office, ANS, 555 North Kensington Avenue, La Grange Park, IL 60526, with a copy to George Miley at 100 NEL, University of Illinois, 103 S. Goodwin Avenue, Urbana, IL 61801. If you have questions, please contact George Miley at g-miley@uiuc.edu or (217) 333-3772.

# Calendar

**Hybrid Vehicles 2000 Conference**
September 11-13, 2000
**Windsor, Ontario, Canada**

**Nuclear Energy in Central Europe 2000**
September 11-14, 2000
**Bled, Slovenia**

**Forum on Converting to a Hydrogen Economy**
September 22-24, 2000
Fort Collins, Colorado

**Texas Renewable Energy Roundup Green Living and Sustainability Fair**
September 29-October 1
Fredericksburg, Texas

**Eighth Russian Conference on Cold Nuclear Transmutation**
October 4-11, 2000
Dagomys, Sochi, Russia

**American Nuclear Society Meeting**
November 12-17, 2000
Washington, D.C.

**Third Asia Pacific Conference on Sustainable Energy and Environmental Technologies**
December 3-6, 2000
Kowloon, Hong Kong

## Eighth Russian Conference on Cold Nuclear Transmutation

**October 4-11, 2000**
Dagomys, Sochi, Russia

$900 includes registration, hotel, meals, proceedings, and transportation to and from airport. (Reduced $800 rate if reservations received prior to August 20.)

RCCNT-8 Organizing Committee, Kurchatov Sq. 1
Moscow, 123182 Russia

Email: bazhutov@erzion.madi.msk.su
gnedenko@kiae.ru

## Third Asia Pacific Conference on Sustainable Energy and Environmental Technologies

December 3 - 6, 2000
Kowloon Shangri-La Hotel, Hong Kong

Internet: http://www.ust.hk/apc2000
Email: apc2000@ust.hk

APCSEET 2000 Secretariat
Department of Chemical Engineering
Hong Kong University of Science and Technology
Clear Water Bay, Kowloon, Hong Kong

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# A Critical Evaluation of the Pons-Fleischmann Effect: Part 2

Edmund Storms*

**Abstract**

Many new studies are available to make an objective evaluation of the Pons-Fleischmann effect possible. The phenomenon is conventionally known as "cold fusion,"or "chemically assisted nuclear reactions (CANR)" when the environment is emphasized, or "low-energy nuclear-reactions (LENR)" if emphasis is placed on the process. A wide range of observations involving anomalous production of energy as well as nuclear products have been published. While many of the claims are still open to interpretation, the general conclusion is that an important, novel phenomenon has been discovered which deserves renewed interest.

**1. Introduction**

This is the second part of a review of the Pons-Fleischmann effect. The first part appeared in the previous issue of *Infinite Energy* and discussed claims for anomalous energy. This part will address claims for helium production as the source of this energy. Various explanations are briefly described to give the reader insight into the proposed mechanisms for the effect. I would like to apologize to the reader for having to use so many references to several proceedings of the International Conference on Cold Fusion (ICCF), which are not easily available. (*Infinite Energy* has all proceedings; please call for information if you are doing research.) Unfortunately, much of this work has not been published in conventional journals in spite of attempts by various authors.

**2. Nuclear Product Production**

Professors Pons and Fleischmann proposed that the source of energy was nuclear fusion.[1] This explanation was quickly rejected when the expected high neutron emission was not found. Tritium production would also be expected. This product has been detected occasionally at significant levels,[2] but the amount does not account for the observed energy. Clearly, anomalous energy is not a product of conventional fusion, nor does it show the behavior found during "hot fusion."

Nevertheless, anomalous emissions have been detected on numerous occasions, including neutrons, X-rays, γ-rays, charged particles, as well as radiation from radioactive products. While such radiation, along with production of non-radioactive isotopes, suggest anomalous nuclear activity, this paper will not attempt to assess these claims. Instead, only claims for helium production will be addressed.

Helium can be produced by several nuclear reactions besides fusion. This element was looked for and found by numerous investigators in several different environments including in the gas,[3-8] dissolved in the materials,[9-15] and emitted as charged particles[16-18] using palladium or titanium.[19,20] Naturally, not all studies are definitive and some failed to find helium when it was sought. Recent studies of gas-loaded palladium powder also claim to show helium production.[14,21-23] Cycling of $LaNi_5$[24] in $D_2$ and allowing palladium-black[14] to remain in the gas are claimed to produce $^3He$ as well as $^4He$. This $^3He$ might actually result from decay of produced tritium.

While these observations are suggestive, only two independent measurements have provided a quantitative relationship between production of anomalous power and helium-4. Both studies used all-metal systems and measured helium in the flowing gas generated during continuous electrolysis. This method insures that air contamination and absorbed He are removed. Two different calorimeter types were used and helium was measured at two different laboratories. These two studies are compared in Figure 1. Three conclusions can be drawn from the figure. First, the two studies agree very well, given the difficulty of the measurement. Second, values for the ratio of the He production rate and power production are largely independent of observed anomalous power, as would be expected if the two quantities are functionally related. Third, the average values are within a factor of two of being consistent with an energy of 24 MeV/helium atom, the calculated value based on mass change. In addition, Bush *et al.*[4] as well as Gozzi *et al.*[6] found helium to be released slowly and only after a delay. Consequently, some of the helium might still remain within the solid palladium deuteride, which was not analyzed, thereby making the measured energy/atom potentially more consistent with a conventional fusion reaction. During the study by Miles *et al.*,[3] seven cells using pure palladium produced no detectable excess energy in addition to the six successful experiments. In each case, no helium over the background value was found in these cells. Thus, anomalous helium was found only when anomalous heat was detected. Only one cell, which used a Pd-Ce alloy, showed heat but no released helium. Such behavior is strongly against chance alone, as Miles noted.

A 23.8 MeV gamma emission has been detected when the helium producing branch of the fusion reaction has been previously observed using conventional fusion. This radiation is required because fusion of two deuterons produces only one product nucleus. Gamma emission allows momentum to be conserved. Because this gamma energy is not detected during anomalous energy production, most critics dismiss the claimed helium as being an artifact. The other two branches of the fusion reaction, which provide the main activity under conventional conditions, are apparently not the source of significant energy in this environment. This distortion of expectation also adds to the skepticism.

What could be wrong with the helium measurement that would justify such skepticism?

1) The results are very scattered and represent a narrow

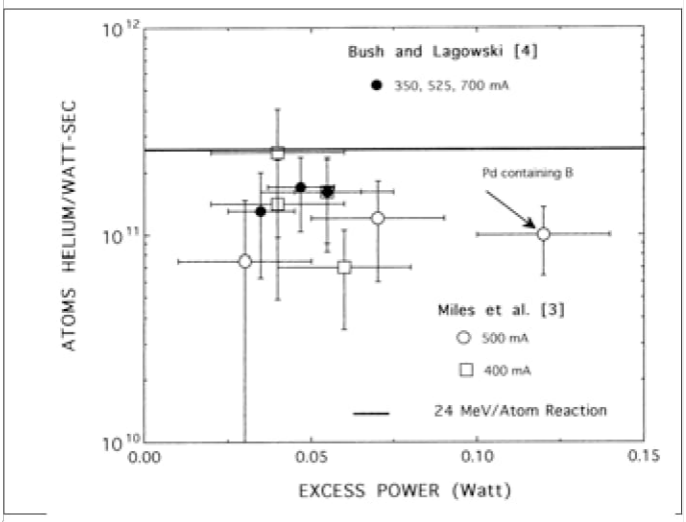


**Figure 1.** Relationship between excess power and the number of helium atoms per watt-sec of energy. The two indicated studies were done independently using different calorimeters and methods for detecting helium.



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range of excess energy and helium. An off-set error in both measurements could generate the proposed relationship by chance.

Answer: Error bars are shown which reflect the expected chance variation including any bias. All of the points agree within their expected error. While excess energy has a narrow range of plausible bias, the helium values could, in principle, range over several orders of magnitude, depending on the care used. Yet, the helium values are well-clustered.

2) The helium concentrations are very small and well below ambient helium concentrations in the surrounding air. A small leak or direct helium diffusion might allow helium to enter the gas stream.

Answer: Metal systems were used which allow insignificant helium diffusion. However, some gasket materials, including a few metals, allow diffusion of helium. This problem was recognized and eliminated. Leaks would admit other elements from the air, such as argon. When this gas was looked for in the mass spectrum, it was not found. In addition, leaks would be expected to be very erratic, thereby leading to a very wide scatter in helium values. This kind of scatter is not seen.

3) The masses of $D_2$ and He are so close that a variable presence of $D_2$ could introduce an error in the He peak.

Answer: In each case, $D_2$ was removed by an absorption trap and, in addition, the mass spectrometers were able to completely resolve the two mass peaks.

4) The excess energy measurements were done using inaccurate calorimeters. While anomalous energy might be real, its measurement during the helium studies might be faulty. Therefore, the claimed correlation may not be real.

Answer: The studies by Miles *et al.*[3] were based on using a double-walled isoperibolic calorimeter that did not contain a recombiner. No recombination was detected. Although the amount of excess power is small, it is well within the claimed accuracy and stability of the device. In addition, failure to detect excess energy correlated with a failure to detect helium. In other words, when heat was present, helium was present, and when heat was absent, helium was absent. A very stable Seebeck-type calorimeter[25] was used during the work by Bush. Again, the claimed excess power was well within the sensitivity of the device.

**3. Attempts to Find an Explanation**

3.1 Basic Properties

Before an explanation is attempted, several basic properties of β-PdD must be taken into account. The β-PdD phase has a face-centered cubic structure (fcc) with deuterium atoms occupying random positions within the deuterium sublattice under ideal conditions. However, composition gradients, impurities, and dislocations make actual occupancy very nonrandom. These atoms are sometimes described as occupying octahedral positions. Tetrahedral positions are not occupied except perhaps during diffusion. The lattice parameter versus composition extrapolates to 0.4084 nm at $PdD_{1.0}$ with smaller values at lower compositions.[26] The lower limit of the β-phase is near $PdD_{0.70}$, depending on the temperature and applied $D_2$ pressure, while the upper limit approaches $PdD_{1.0}$ as all vacant positions are filled. In addition to this nonuniformity in composition, palladium can also acquire many dislocations in the palladium sublattice, which can easily degrade to microcracks. Simply bending a wire will cause these dislocation to form in great abundance. In addition, the hydriding process will change the crystal morphology and introduce strain. When electrolysis is used to form the hydride, many impurities are also introduced into the lattice, making the surface region a very complex alloy containing at least Li, Pt, Si, and B, with unknown characteristics. Such factors combine to make the material very nonuniform in all its properties and very unlikely to be described adequately by "ideal" models.

As the deuterium content is increased, the equilibrium pressure of deuterium gas also increases, as shown in Figure 2.[27] The enormous pressure required to form the required large deuterium concentrations is the main reason these compositions are so difficult to achieve by electrolysis. Deuterium can readily leak out of the sample through the many cracks known to inhabit such material. Notice that the pressure increases as temperature increases, with no indication of a reverse temperature effect near $PdD_{0.85}$ as proposed by Fleischmann[28] to explain energy production at higher temperatures. Indeed, all available data, as well as the theory of such compounds, predicts that all compositions of β-PdD will have a higher equilibrium pressure of $D_2$, hence lose deuterium more rapidly, as temperature is increased. Consequently, either excess energy production does not involve the β-phase as argued by Storms,[34] or the effect does not require a high composition at higher temperatures, as argued by Waisman and Summeri.[29]

Occasional reports are published claiming average compositions in excess of $PdD_{1.0}$.[30-32] These high compositions are proposed to be caused by deuterium ions occupying tetrahedral sites[33] or by formation of a new phase based on $D_2$ ions occupying octahedral sites.[34] The latter explanation would imply a two-phase region between $PdD_{1-x}$ and $PdD_{2-y}$ where x and y are small unknown numbers. Recent measurements indicate that this high composition forms in the near surface region and is required before anomalous energy is detected. In brief, the nuclear reactions do not occur in β-PdD, but in an unknown phase of higher deuterium content containing various impurities. Therefore all models which depend on the properties of pure β-PdD must be viewed with skepticism. This trans-beta phase, once formed, can be proposed to become more stable as temperature is increased, thereby explaining the claimed excess energy obtained at higher temperatures. The roles of the always present impurities, such as Li and Pt, are not known.

On the other hand, the nuclear-active environment may not involve palladium at all. Recent claims for heat production using gold[35] or platinum,[36-38] neither of which absorb significant hydrogen,[39] suggest the active environment is a deposited layer on the surface. While more than one active environment might exist, another possibility must be considered, *i.e.* that all such nuclear reactions occur in the same or similar chemical environments that are created as a surface layer on an otherwise inert substrate. When used with the electrolytic method, palladium may turn out to be a poor choice of substrate because of its tendency to crack in the presence of hydrogen.

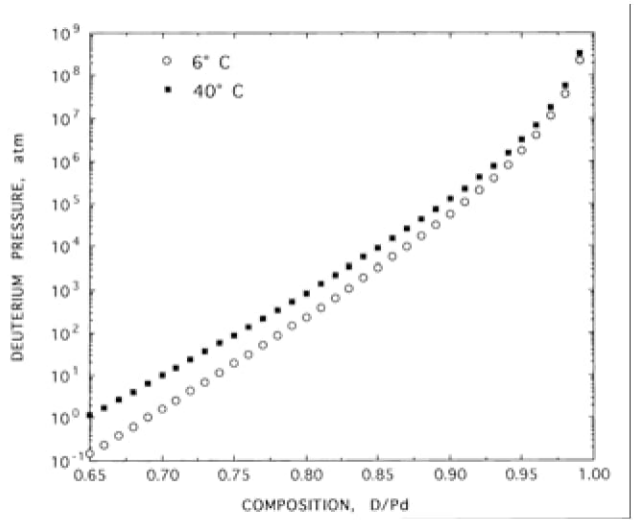


**Figure 2.** Equilibrium pressure of $D_2$ gas over PdD for various compositions at 6°C and 40°C.

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3.2 Requirements of a Theory

Theoretical objections to the claims are based on the following arguments as summarized by Huizenga[40] and many other skeptics:

1) The deuterium nuclei in PdD are not close enough to allow nuclear interaction.

2) Energy required to overcome a Coulomb barrier is not available in a chemical compound.

3) If a fusion reaction should occur, the nuclear products must be the same as those observed when fusion is initiated using high energy methods. These products are not detected with the expected magnitude or in the expected ratio.

4) Any helium produced by a fusion reaction must be accompanied by gamma emission, which is not detected.

5) The resulting energetic nuclear products should produce intense X-ray emission, which is not detected.

The issue requiring resolution is whether an unknown process exists which would allow fusion or any other nuclear reaction to occur in the unique environment of a chemical lattice. Adding to the challenge are the many other types of nuclear reactions now being claimed. A few attempts to understand the process are discussed, but only to give the reader insight into the types of mechanisms being considered. A more detailed evaluation is not possible at the present time for various reasons. For example, the author finds that theoreticians seldom agree with each other, preferring instead to focus on their own favorite models. Consequently, agreement or at least objective discussion within the field is handicapped from the start. Partial evaluations have been undertaken by Preparata[41] and by Rabinowitz *et al.*,[42] but with very little agreement. In addition, there is a tendency to choose only those experimental observations that fit the model while ignoring much data that are inconsistent. This makes an application of the model very limited. This review will only attempt to show the general approach and note whether the proposed models are consistent with experimental observation. Only models which have been developed to a high degree are cited. Many dozens of models, which involve only brief suggestions or address limited observations, are not included. The fractofusion model, although it has been given considerable attention, is not discussed because this process involves high energy and is expected to produce "normal" nuclear products, hence is not consistent with the anomalous observations.

Models generally fall into two general overlapping categories. The first is a mechanism that explains how a nuclear reaction can occur once the necessary conditions are achieved, and the second is the nature of the necessary conditions. The first category involves nuclear physics and the second one involves chemistry. Both must be addressed by a successful theory.

To be successful, a theory must answer at least five basic questions to explain the P-F effect and several other questions if the entire range of published observation is to be explained.

1) What mechanism allows the Coulomb barrier to be overcome? This question is fundamental. The model will have to explain the proposed fusion between deuterium nuclei as well as how nuclei as heavy as palladium can suffer a reaction with nuclei as heavy as oxygen.

2) What mechanism distributes the released energy throughout the lattice rather than requiring it to be focused on a few individual particles? This mechanism must also explain why some nuclear energy is retained by the nuclear products when these products are produced very near a surface. Otherwise, charged particles having significant energy to leave the material would not be detected.

3) How is the proposed mechanism related to the physical environment? Most present theories assume the nuclear reactions occur in β-PdD having a composition near $PdD^{1.0}$. The model must explain why anomalous reactions occasionally involve other materials, why the required conditions are so difficult to achieve, and why the active regions are so localized on the surface.

4) What nuclear reaction is the source of observed helium? Fusion is not the only conceivable source of helium as a nuclear product.

5) If helium results from a fusion reaction, what mechanism allows conservation of momentum and energy, and what mechanism distorts the reaction paths to produce helium rather than neutrons and tritium?

A single explanation seems impossible. The diverse nature of observed behavior and uncertain reality of the claims add to this problem. To simplify the challenge, this review will focus only on the environment claiming to produce energy when heavy water and palladium are used in an electrolytic cell. Any successful general theory will have to explain how this anomalous energy is produced in other metals while using other methods, involving both light water and heavy water. In addition, the model would have to explain how the many anomalous heavy isotopes are produced.

A quantitative test of any model is impossible using experimental data, because the palladium samples are very nonuniform, hence the fraction of active material is unknown. Indeed, a calculated power density based on the gross physical dimensions of the sample will greatly underestimate actual local power density, and the ultimate power density of which this phenomenon is capable. Therefore, comparing samples on the basis of watts/$cm^2$ or watts/$cm^3$, as is frequently done, is not appropriate. Indeed, evidence of local melting suggests that very high local power density is possible.

The models discussed below are placed in eight general categories, but with the understanding that to be successful, a model may have to invoke more than one category. In addition, the examples chosen are intended to give the reader only a very general understanding of the range of models being proposed. The discussion is not meant to be a complete evaluation of all efforts to understand the phenomenon.

3.3 Proposed Theories

3.3.1 Neutrons

The easiest way to solve the Coulomb barrier problem is to invoke the neutral neutron. However, free neutrons are unstable, hence are not present in sufficient numbers within a solid. Consequently, such theories are forced to identify a steady source of virtual neutrons.[43-45] This search has taken several forms. Kozima[43] proposes that trapped thermal neutrons catalyze the cold fusion reaction (TNCF Model). In an attempt to identify a source of such neutrons, he has proposed formation of metastable neutron clusters of various sizes. Several questions have not been answered, including exactly what conditions cause the clusters to interact with the surrounding nuclear environment, why individual neutrons are not emitted from the material during such interaction, and why the clusters do not produce "normal" nuclear products. In addition, his efforts to compare the concentration of these neutrons to the observed effects is doomed to failure because nuclear activity is highly localized and impossible to relate to the measured volume of the sample, a variable used in his model.

On the other hand, several workers have proposed direct formation through interaction between a proton or deuteron and an electron. However, complete formation of a neutron requires energy as well as a neutrino or antineutrino, both of which are not present in sufficient amounts. In addition, reaction with a neutron should produce "normal" nuclear products, which are not observed. To eliminate these problems, the neutron is proposed to exist only as a partially collapsed structure. The manner of this partial collapse has been addressed by several workers and the process is proposed to produce various structures called Hydrex,[47] Itons,[48] Hydron,[49] and Hydrino.[50] This "virtual" neutron can, under special conditions, presumably be

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absorbed by a nucleus in the same manner as a real one, while producing anomalous nuclear products in the process. The source of the required neutrino has not been clearly identified. Mills views the process of partial electron collapse to release energy without involving a nuclear reaction[50] while Dufour, using conventional theory, finds that the collapse requires energy. The region between partial collapse and complete reaction between an electron and a proton is a gray area in which many models can operate. As yet insufficient experimental information is available to decide which model or models are correct.

Dufour[47] has gone one step further in describing how his collapsed hydrogen, the Hydrex, interacts with the nuclear environment. Rather than causing fusion, the structure is proposed to attach itself through dipole interaction to a large nucleus such as palladium, a process which is proposed to reduce the energy barrier for release of helium-4. This process would predict energy production in the 5 MeV region which, so far, is at odds with observation. Dufour must assume that considerable helium which has been missed still remains trapped in the Pd metal .

Another approach involves the existence of multi-neutron structures which are stable. The existence of di-neutrons has been proposed before the CANR phenomenon needed such a model. Fisher (Thomas Paine Associates)[51] has carried this idea one step further. He proposes that large, stable neutron clusters can form and that these can attach themselves to normal nuclei to produce super-heavy atoms. A small concentration of such atoms is proposed to be present in all matter. Under the right conditions, these neutron clusters are released, thereby causing novel nuclear reactions. Within the confines of the assumptions, the model can explain many observations of CANR. In addition, the work of Oriani[52] suggests the existence of super-heavy carbon in electrodes subjected to CANR processes.

3.3.2 Novel Particles

Various exotic particles, which have the ability to catalyze nuclear interaction, are proposed to exist in nature. These are located within the structure as part of the normal constituents or are provided by a steady flux from cosmic sources. The models do not show why hard-to-create special chemical conditions are required for these particles to do their unique work. These particles are called Neutrium,[53] Quark,[54] Hemitrons,[55] Muon Neutrinos,[56] or Erzion.[57]

3.3.3. Electron Structure

Various modifications to the electron structure,[58-61] including impurities,[62] stress,[63] diffusion,[64] oscillation,[65,66] applied current,[67] or changes in the periodicity of the atomic structure[68,69] are proposed to reduce the Coulomb barrier. The QED[33] theory addresses the latter process in some detail. In this case, normal electrons within a lattice are proposed to acquire a coherent structure, much like a superconductor, which can reduce the Coulomb barrier and carry away the resulting nuclear energy.

Work based on the swimming electron layer model[70] is ongoing at the University of Illinois under the direction of Professor George Miley. This model proposes that deuterons can exist as bare nuclei and move freely throughout the lattice. Relatively unbound electrons located at a metal surface or between those metals which have much different Fermi energies can screen the Coulomb barrier between bare deuterons or between them and other atoms. The result is enhanced fusion and various transmutation reactions that produce energy and helium.

While these processes are proposed to initiate nuclear reactions, they do not address which reactions might result.

3.3.4 Phonons

Lattice vibrations are proposed to cause a few adjacent deuterons to get close enough to fuse,[71,72] especially in the double layer at the electrolyzing surface.[73] The likelihood of this process is increased by the average atom position in the deuteride being closer than previously thought.[74,75,66] Kucherov,[76] working at ENECO (Salt Lake City, Utah), proposes that these vibrations can transfer incremental energy to the nucleus until sufficient instability is accumulated to produce alpha emission or other nuclear reactions. Hagelstein (MIT)[72] is also exploring lattice-induced alpha decay of various heavy-metal impurities as the source of energy and helium, but using a different model from Kucherov. His model has evolved to the stage where quantitative predictions can be made about the energy of emitted particles and radiation. Li *et al.*,[77] working in China, propose a resonance between energy levels in the nucleus and those in the lattice allowing energy transfer to a d-d pair. Energy can be transferred into the lattice by the same process after fusion takes place.

3.3.5 Particle-wave Transformation

Under special conditions, the deuterons dissolved in a periodic lattice are proposed to assume wave-like properties which permit fusion and deposition of the resulting energy throughout the lattice.[78-80] The Ion Band State Theory is explored in detail by Scott Chubb (Naval Research Laboratory) and Talbot Chubb (Oakton International Corporation).[81] They assume that energy is transferred in steps from the wave-states of two deuterons until the wave-state of a helium nuclei is achieved. Once this energy is lost to the other wave-states, the helium wave can revert back to a particle and be detected as such. This idea addresses problems associated with d-d fusion, but it does not explain other claimed nuclear reactions.

3.3.6 Nonfusion Nuclear Reactions

Passell (EPRI, ret.)[82] proposes that helium and energy are produced by the reaction $^{10}B + {}^{2}H = {}^{4}He + {}^{8}Be = 3\ {}^{4}He$, giving 5.9 MeV/He atom. Although this reaction might occur under some conditions, the expected energy is not consistent with the measurements shown in Figure 1.

Takahashi (Osaka University)[16] has proposed a reaction between deuteron clusters containing three or four atoms, two atoms of which fuse while the remaining atoms help carry away the resulting energy. Various reactions between these deuteron clusters and other elements, producing unstable isotopes of Li and Be, are also proposed to result in helium production. This model avoids the problem of momentum conversation during fusion, which requires gamma emission.

Enhanced electron screening is proposed by Hora (University of New South Wales)[70] to allow reactions such as $Pd + d = {}^{103}Rh + {}^{4}He$, which releases 10.5 MeV of energy. This and other nuclear reactions[83,84] initiated by this process are proposed to have a large positive temperature coefficient, thereby explaining the greater effect observed at higher temperatures.

3.3.7 Nuclear Structure

The nucleus is proposed to have a structure which facilitates nuclear interaction, being composed of clusters of nucleons which can be easily lost or gained.[85-87] These clusters are thought to leave or enter the nucleus more easily than expected once the barrier is overcome.

3.3.8 Reduced Barrier

The Gamow factor has been explored in different ways by Kim (Purdue University) and McNeil (Colorado School of Mines) to see if unanticipated conditions can cause a reduction,[88,89] including a tunneling mechanism.[90-92] Apparently, the barrier is lower than previous calculations would predict, but not as low as required to explain the claimed observations. Measurements of the branching ratio and the fusion rate when very low $D^+$ energies are used to bombard various metals indicate that electron screening can, indeed, lower the barrier in metals.[93]

**4. Discussion**

Before the claimed energy production can be attributed to fusion, nuclear products need to be detected. The expected neu-

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tron emission has been detected as large bursts and as a small steady rate, but in too small an amount to account for anomalous energy. The expected tritium has also been detected, but again in very small amounts. Only helium has been found in sufficient quantity to account for the claimed anomalous energy. In addition, both tritium and helium seem to be born with very little translational energy, again in contrast to hot fusion. While this helium might result from various prosaic sources, the list of observations is growing and the detected amounts are increasing, making such explanations increasingly unlikely.

Although production of various heavy isotopes (transmutation) has not been addressed here, this is another growing aspect of the phenomenon which needs to be understood. Not only are such reactions a potential source of energy, but they present a significant challenge to any explanation.

If energy does result from a novel form of fusion, how can this be explained? Unfortunately, we are presented with too many explanations rather than too few. To make matters worse, the nature of the nuclear-active regions has not been identified. Without such information, duplication of the effect is difficult and any theory is based more than usual on imagination. Identification of the active region is handicapped because these regions occupy only small areas on the surface and are unstable. Their existence is seen only after a few have destroyed themselves by melting the surrounding material. This problem does not have an easy solution. In any case, the conventional focus on β-PdD as the nuclear-active material needs to be re-examined, because the properties of this material are not consistent with observed behavior.

In general, the various explanations have not been very useful because they fail to identify the unique conditions which must exist within the material to make it become nuclear-active. It does no good to say, for example, that a source of virtual neutrons must be present, that the electrons must be coherent, or that a particle-wave conversion must take place. The question is, how can these events be made to occur? Without such knowledge, efforts to make the effect more reproducible will be based more on luck than on knowledge. At the very least, to be useful an explanation must identify those real-world variables that influence the effect and show how the necessary conditions can be made less rare.

Once the necessary conditions have been identified, a successful theory must answer several difficult questions about the nature of the nuclear reactions—in brief the three "miracles" proposed by Huizenga.[40] How can a Coulomb barrier be overcome using the very limited energy available within a lattice? How can the released energy be communicated to the lattice in the brief time frame typical of such events? What process distorts the fusion branches to favor helium production? What causes different nuclear products to be produced under what appear to be similar conditions, including those resulting from transmutation reactions? In addition, any proposed novel process must be tested outside of the CANR field, *i.e.* it must be shown to work under other conditions. As yet, no proposed theory has met the challenge, although a few are getting close.

**5. Conclusion**

The claims for anomalous energy production using electrolysis of heavy water have been evaluated and found to have a high probability of being caused by a novel phenomenon. (See Part 1 in *IE*, No. 31.) In addition, the most likely source of the heat is a nuclear reaction which produces helium. This nuclear reaction is not normal fusion and does not follow the rules required by conventional theory. Numerous models have been proposed to explain the observations, but none at the present time can account for all of the reported behaviors. More work is required to determine which of the behaviors are part of this novel phenomenon and which can be explained by ordinary processes. However, the claims have now reached a level of understanding which justifies a reexamination of the published work and serious attention by the scientific community.

A standard has been applied to this field which is unique to modern science and which sets a precedent to hinder acceptance of future novel claims in nuclear physics. This standard involves various outspoken skeptics demanding that nuclear products be identified as the source of the claimed energy before they will even accept that the anomalous energy is real. Moreover, these nuclear products had to fit the behavior found during "hot" fusion. In other words, if the behavior is not as expected, the entire set of observations is rejected. Application of such a limited and uncompromising approach is not how science has advanced so successfully in the past. Finally, after eleven years of work, a nuclear source for the anomalous energy has been demonstrated. It is now incumbent upon these same skeptics to acknowledge this work and join in an effort to evaluate the new discoveries in a rational, open-minded, and productive manner. The integrity of these people is now on the line and their reputations as competent scientists may eventually be at risk.

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***Storms continued on page 70***

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# Hydrogen as the Driver of Global Tectonics

**C. Warren Hunt***

**Abstract**

Hydrogen in Earth's core has been regarded with skepticism as inconsistent with the orthodox view of Earth's origin by the accretion of planetesimals. The volatility of hydrogen has been seen to preclude its preservation under the molten Earth conditions that were believed to have followed planetary accretion.

New data from a drill hole at Fort McMurray, Alberta, Canada, gives the contrary picture—pervasive hydrogen in shield granite. $H_2$, $CH_4$, and $CO_2$ were found to evolve continuously from the shield granite as it was pulverized by drilling, their relative levels varying erratically. These findings call into question the premise of the molten Earth and raise the question of what influence hydrogen might have had on endogenic tectonics.

Hydrogen combines chemically with all elements but the noble gases. It penetrates crystal structures of minerals and at high pressure forces its nuclei inside the first electron rings of metal atoms. The chemical and atomic hydrides are densified and fluidic. If pressure is released, hydrogen is emitted and crystalline structure and density return to normal. The densification and de-densification represented by hydride behavior on a regional scale causes isostasy. Its positive form is emergence and mountain growth; its negative form is subsidence and rift or trench deepening. Both responses reflect isostatic imbalances that develop after migration of hydrides. Mass shift—surficial or interior—effects relocations of Earth's axis of rotation.

Chemical energy is stored in the Earth's interior in molecular forms; silicon carbide and silicides of various metals are abundant examples. When hydrogen dissociates silicides, their energy is transferred to the resulting hydrides. Thus, hydrogen reacting with silicon carbide, SiC, produces $SiH_4$ and $CH_4$ (silane and methane), gaseous compounds loaded with the stored chemical energy from the parent mineral. These gases then carry silicon and energy to crustal levels. Silanes oxidize in the presence of the first water they encounter, releasing their energy and silicon as volcanic effusives or passively by creating granites, the substances of continents. Hydrocarbons, mainly methane, migrating with the silanes are less reactive and accumulate where barriers obstruct their upward progress, thus generating deposits of natural gas, petroleum, and coal, the principal "*energy minerals*." Hydrogen in shield granites is a natural residue after silane oxidation.

Endogeny, whether violent, as in volcanism and earthquakes, or quiescent, as in metasomatic replacement and isostatic adjustment, is the consequence of hydride systematics. *Hydrogen is the driver of global tectonics.*

## Hydrogen Systematics

Hydrogen is the *first element* of the universe, the fundamental building block for all other elements. It carries energy itself and mobilizes energy latent in the molecules of other elements, especially silicon and carbon. Hydrides of many non-metals are familiar molecular compounds: water, ammonia, hydrogen sulphide, the acids, etc. In the case of the metals, hydrogen makes hydrides in four generically different states. These are shown in Table 1.

The presence of pervasive hydrogen in shield granite has escaped previous recognition, perhaps due to lack of suitable instrumentation, perhaps by oversight. Profound geological consequences follow from an interlink between the presence of hydrogen in Earth's interior and tectonism on Earth's surface. They result from the extraordinary properties of the "*first element*." Typical evidence of the occurrence of hydrogen and methane in the crust is shown by the mudlogging on a drillhole at Fort McMurray, Alberta, Canada. Values shown are only relative, being derived by aspiration of the drilling mud. (See Figure 1.)

The foregoing hydride states were dramatically illustrated by Vladimir N. Larin in his book, *Hydridic Earth*.[1] Figure 2 shows solid potassium subjected to a range of pressures in air and then in an atmosphere of hydrogen. Starting with density of ~0.87, pure potassium (K) in air compresses along a smooth curve to ρ= ~2.3 at 25 GPa. By contrast, K in an atmosphere of hydrogen absorbs H up to ρ = 1.6 without pressure increase. Then, with pressure increase of about 4 GPa, K is densified to ρ = 1.9, at which point the K absorbs H without further pressure increase

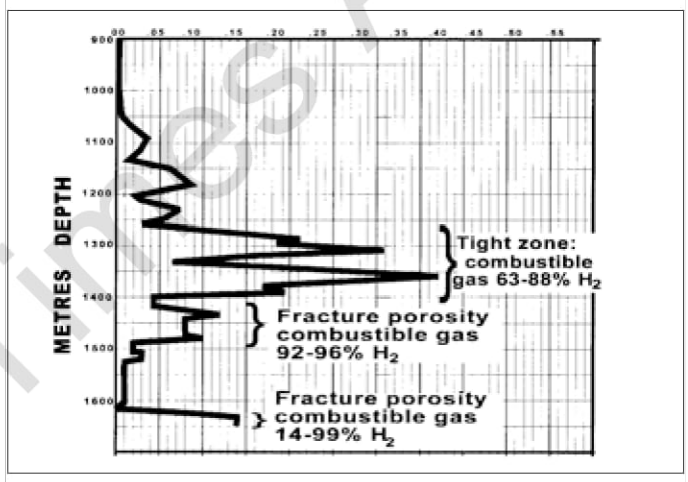


**Figure 1.** Ratio of hydrocarbon to total combustible gas in the AOC Granite 7-32-89-10 well.

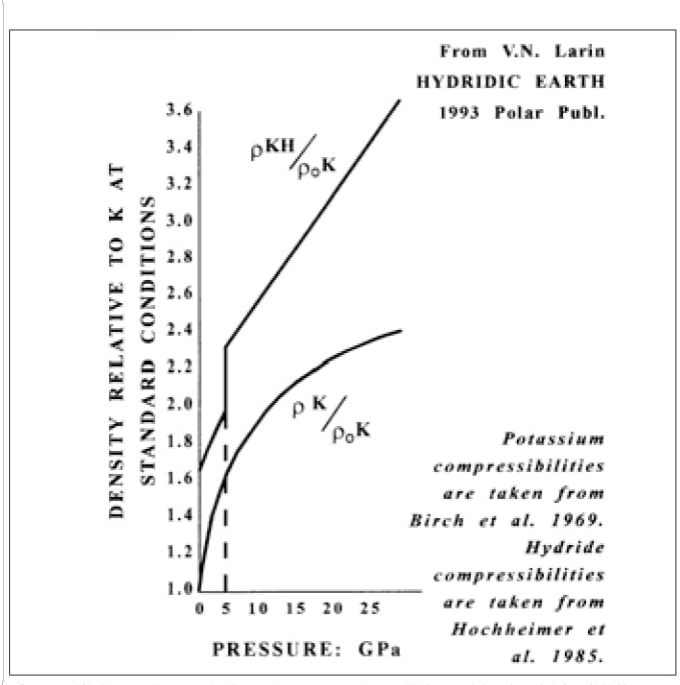


**Figure 2.** Density variations in potassium (K) and its hydride (KH) over wide ranges of pressure.



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up to $\rho = 2.3$. From there the K densifies along a straight line until $\rho = 3.6$ at 25 GPa. Maximal possible loading of K with H is not known.

Larin uses the term *"intermetal"* to designate a metal densified by hydrogen, and he implies that other metals will show similar behavior in the presence of hydrogen at elevated pressures. Therefore, lithospheric density should vary in response to the level of hydrogen pervasion, depth below the surface, and the relative fluidity of the different hydrogen-impregnated metal atoms that are involved. Let us consider some of the variables.

*Hydrides in all their four states have the effect of densifying metal fabrics*. Maximal densification occurs with the fourth state, where high pressures inject hydrogen nuclei into atom shells and create denser atoms, intermetals. If pressure decreases on an intermetal, hydrogen is evolved and normal atomic structure is restored.

The gravitational affects of densification and de-densification therefore may induce either isostatic subsidence or uplift, thereby either increasing or relieving lithospheric stress. As hydrogen is breathed in or out of mineral fabrics, Earth *"respires"* and isostasy adjusts the surface accordingly.

Hydrides are fluids in most natural habitats. This is true at standard conditions for gaseous hydrogen compounds of the non-metals, such as water, hydrochloric acid, ammonia, and hydrogen sulphide. It is also true for the hydrides of the transition elements, which are dealt with in the next paragraph. At pressures of Earth's interior the metal hydrides are also fluid. Even hydrides of refractory metals such as titanium become formable when transformed to *intermetal*. With a melting point of 1800°C and low heat conductivity, titanium metal, which cannot be machined, behaves like a paste after injection of hydrogen at high pressure but low (~300°C) temperature. Crystalline integrity is restored when hydrogen escapes after pressure reduction.

**Table 1.** The Four Hydride States

| | |
|---|---|
| I - COVALENCE | $H_2$, $H_2O$, $CH_4$, $SiH_4$, $NH_3$, etc. |
| II - INTRA-LATTICE ADSORPTION | A physical association. H atom retains electron while in metal lattice |
| III - INTRA-LATTICE OCCLUSION | Electron-sharing. $H^+$proton sheds electron within metal lattice. |
| IV - IONIC HYDRIDES | $H^+$ proton penetrates first electron shell. High pressure, densified atom makes "phase-changed," new substance & $H^+$, hydronium ion. |

**Table 2.** Crustal Reaction of the Hydrides of Silicon and Carbon

HYDROGEN-SILICON CARBIDE REACTIVITY

$$4H_2 + SiC \rightarrow SiH_4 + CH_4$$

THE SILANE REACTION WITH WATER

$$SiH_4 + 2H_2O \rightarrow SiO_2 + 4H_2$$

THE OXIDATION PRODUCTS OF METHANE AND SILANE UNDER CONDITIONS OF OXYGEN SCARCITY

$$CH_4 + O_2 \rightarrow 2H_2O + C$$
$$SiH_4 + O_2 \rightarrow 2H_2 + SiO_2$$

THE OXIDATION PRODUCTS OF METHANE AND SILANE UNDER CONDITIONS OF OXYGEN ABUNDANCE

$$CH_4 + 2O_2 \rightarrow 2H_2O + CO_2$$
$$SiH_4 + 2O_2 \rightarrow 2H_2O + SiO_2$$

THE EVOLUTION OF HEAT FROM METHANE AND SILANE OXIDATION

$$CH_4 + 2O_2 \rightarrow 2H_2O + CO_2$$

Heat: -19.1 + 94.4 + 268.4 = 212.1 kCal/80 mol-eq-wt

$$SiH_4 + 2O_2 \rightarrow 2H_2O + SiO_2$$

Heat: -11.9 + 201.3 + 268.4 = 326.2 kCal/96 mol-eq-wt

*Silanes are the hydrides of silicon, and hydrocarbons are the hydrides of carbon*. The hydrides of *transition elements* are special cases among hydrides. Silicon, carbon, and germanium are the *transition elements*, neither metal nor non-metal, sometimes acting as metals, sometimes as non-metals. In 1997 the author advanced his Anhydride Theory explaining petroleum as a mixture of *anhydrides* of the methane that effuses constantly from Earth's interior.[2,3,4] In essence, the author's concept is that hyperthermophyllic bacteria and archaea strip hydrogen from methane and progressively create larger hydrocarbon molecules up to the point where there is no more hydrogen present and pure carbon is deposited in peat to create coal. Methane and silane, thus, have a common origin and usually are both present in effusing inner Earth gas, although in widely varying proportions.

The reactions of Si and its sister element, C, are shown in Table 2.

### Volcanism and Magmatism

The silanes, like the low carbon numbered alkanes (methane, ethane, etc.), are all gaseous at room temperatures. They react violently with oxygen, even taking it away from water. Where water is encountered, the silane reactivity generates silica, which itself may create barriers to further gas migration. The breaching of these barriers follows pressure buildup from incoming gas. Violent breakout results. This is underground volcanism, the phenomenon commonly known as an *earthquake*. The usual explanation of earthquakes is that stress builds up across natural fractures and finally yields catastrophically. However, most earthquake foci are deeper than 5 km, at which depth most rock becomes ductile and stress cannot build up. Thus, earthquakes are volcanic breakouts occurring deep underground and often causing shifting of surficial terrain consequential upon mass relocation at depth.

Where silane permeates mafic rock fabrics, it can substitute silicon for heavy metal (Mg and Fe mainly) in the rock-forming minerals and transforming mafic rock to felsic (granitic) rock. This process can be slow, atom by atom, and hence passive, adding rock volume and diminishing rock weight, squeezing off the pathways for incoming gas, and causing terrain inflation (mountain building). If the inflow is rapid and water is abundant, the heat released from the silanes can cause meltdown of the country rock to magma. The magma may intrude overlying rocks of any kind diapirically and congeal over millennia as plutons and batholiths.

If magma is close enough to the surface to break out, its first eruption is usually its gaseous, phreatic phases, which are accompanied by a rain of siliceous sand and dust generally known as volcanic ash, a primary product of silane combustion. Fiery volcanism is the result of such silane combustion and the secondary combustion of its released hydrogen. After gas emission, the lava follows. Apparently, earthquakes are to be expected at those crustal levels where water is present in rock fractures to react with effusing silanes. The oxidation releases latent heat and deposits metal components from accompanying metal hydrides as metal ores. Where water is not encountered in sufficient quantity deeper in the crust and the silanes reach volcanic conduits, the heat release may be delayed until emission at the crater itself. This results in fiery volcanism and the destructive, often incandescent volcanic blasts known as nuée ardente. The solid product of the silane reaction is the siliceous rain of rock and sand known as *volcanic ash* or *tephra*. These deposits range from boulder fragments in *ash* matrices to pure, white *ash* deposits variously called *"tripoli," "pumice,"* or *"diatomite."* All of these are to be found in profusion blanketing great areas of all continents and ocean bottoms.

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Orthodoxy regards the term *volcanic ash* as a misnomer, because it is not generally recognized that there is a highly combustible gas that will give siliceous sand as a combustion product. It should be apparent that if the volcanic ash were merely abraded fragments plucked from the vent walls that the quantities involved would have resulted in major surface collapse of vent areas. Instead of collapse, most volcanic areas exemplify built-up cones of erupted rock. Thus, the term *ash*, meaning a product of combustion, is exactly the right term to describe the volcanic effusive material. Because silanes react explosively with free oxygen and actively with water, they cannot exist naturally on Earth's surface and have been unsuspected up to now for that reason. Evidence for their presence is nothing less than the super-abundant, aptly named blankets of *volcanic ash* and *tephra*.

### Mass Shifts Within the Earth

Mobility of hydrides in the Earth's interior offers an explanation for mass shifts with tectonic consequences. Shift can be understood as rising toward the surface of fluidic hydrides, or the magmas they create when they react with water. Such rising can remain quiescent or become volcanically violent. Wherever mass shifts within the rotating globe, a compensating shift of the rotating axis must occur. The only mass shift that could happen without a polar shift would be an upward movement exactly on the equator. In addition to mass shifts within the body of Earth, the off-loadings of surface materials by gradual erosion or by the catastrophic, gravity-driven, terrain transfers known as horizontal over-thrusts would cause pole shift, as would external tidal forces. Disequilibrium caused by mass shifting can thus be interpreted as the engine of polar wander.[5]

It is important to differentiate between earthquakes resulting from silane reactions and earthquakes caused by stress release on faults, the latter being the prevailing explanation for all earthquakes. Stress build-up in crustal rock is well known from rock bursts that occur in mines. These explosive occurrences are possible in the brittle rock normal at shallow crustal levels. Deeper in the crust, brittle behavior is replaced by ductile behavior. Below the brittle/ductile transition, overburden pressure causes rock to deform under shear stress and thus to be unable to sustain accumulation of stress. The transition occurs at different depths for different rock types but usually in the range of 3 to 6 km.

Most earthquakes are recorded at 5 to 18 km depths, below the brittle/ductile transition and mining depths. These must therefore be attributed to underground volcanism. Only shallower earthquakes, those above 5 km depth in the brittle rock zone, should be interpreted as events of stress release. The orthodox generality that earthquakes are kinetic expressions of stress release on faults is inappropriate and should be abandoned in favor of a dual interpretation: stress release for shallow ones; hydride volcanism for deeper ones.

### Continents and Earth Expansion

Silicon is the third most abundant element on Earth (after hydrogen and oxygen) and comprises approximately 55% of crustal rock and possibly 30% of upper mantle rock. An age-old geological conundrum has been the mechanism by which granitic continental platforms could have been segregated from the underlying, silica-poor mantle. Where did all the silicon come from and how did it get where it is? The concept of hydride mobilization of silicon at core depths *below* the silica-poor mantle followed by upward migration as volatiles through the mantle and into the crust is an answer.[1,6]

Petrologists interpret the continents, which are substantially underpinned by granite *shields*, as representing cooled magmas. The heat source for magma formation has been taken to have been the kinetic energy of impacting planetesimals, which are thought to have rendered the entire Earth surface molten. As mentioned at the beginning of this paper, the hydridic Earth model is inconsistent with an accretionary origin. Also as mentioned, silane provides abundant latent heat, and it may be released rapidly, as in a reaction to abundant water, or gradually, as in the transformation of mafic rock to felsic rock, atom by atom, as taken up next. Where sufficient silane and sufficient water are present, the combination may evolve sufficient heat to melt the surrounding country rock and create a magma. The magma may flow to the surface as lava following a volcanic eruption, or it may well up without breaking out and congeal as a diapiric pluton or batholith.

Thus, hydride theory sets the stage not only for the development of granites but for intrusive plutonism and extrusive lava flooding on the surface. In the latter case lava is often found as caps above thick deposits of tephra, thus showing that it followed the gaseous tephra event. Where heat evolution is not so great as to create magma, the silica produced by silane combustion may, as already mentioned, block the conduits along which the silanes migrated, thus setting the stage for explosive volcanic breakout and an *earthquake*.

Mafic rock transformation is a replacement process known as metasomatism or granitization through replacement of the metal components, Mg and Fe mainly, of rock minerals by silicon. It is a gradual, *in situ* process. The subject is taken up in detail by L.G. Collins,[7] in *Expanding Geospheres*. He reasons that silane permeation of mafic rock followed by heavy metal replacements with silicon result in the transformation from silica-poor to silica-rich minerals and the addition of quartz to rock fabrics. The mineral suites of mafic rocks are thus transformed from mafic to felsic. The resulting volumes increase, and the resulting densities diminish. Thus, granite may be formed either by melting of country rock and the rise of a pluton, or it may be created atom by atom, the original mafic rock remaining in place and being transformed *in situ* to granite. Several stages of melting, remelting, and transformation may in these ways affect a single granitic body.

Positive isostasy produces scenery when rocks, lightened by silicon substitutions and quartz deposition, rise as mountains. Shield granites, on the other hand, are the lower-lying core components of continents. Generally they are the erosionally-beveled remnants of former positive isostasy. They still stand at elevations far above the oceanic depths and well above the mean elevation of the solid Earth.

Negative isostasy, as already mentioned, results from regional densification and is the product of gaseous hydrogen perva-

**The rise of mountains and the sinking of oceanic trenches, then, are explained by hydride theory without horizontal compressive forces in the crust or the invocations generally called plate tectonics. . . Continents, built over eons by multitudes of atoms of silicon transferred from the interior, are elegant signatures of Earth's past expansion. . .The mass of the Earth does not change, but is shifted outward generally, thus slowing the rotation and reorienting the axis.**

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**In the two worlds, physical and biological, spontaneous organization imprints holograms on populations of atoms, the holograms organize atoms into molecules and the molecules into clusters, crystals, and catalysts. In the world of biology holograms create enormously complex molecules and whole organisms, life forms. All of these holograms in both worlds depend on the internal energy of hydrogen to carry on their self-replicating ability. . .Hydrogen, then can be said to be the provider of the spark of life and the dynamo of the planet.**

sion at high pressure into mafic rocks without silanes, which would tend to de-densify host rock and nullify negative isostasy—subsidence. On regional scales hydrogen infusion explains oceanic deeps and trenches as well as basins on continental platforms. All of these are expressions of negative isostasy.

A subsident region, whether basin, trench, or oceanic deep, is positioned to receive *off-loadings* from adjacent regions of positive isostasy. These *off-loadings* may take several forms: incremental sedimentation by riverine transport, catastrophic volcanic deposition, or giant landslips. The huge landslips of mountain rock known as *over-thrusts* at their inception debouched into basinal depressions either intracontinental depressions, basins, or submarine depressions, embayments, or trenches in the oceans.

Huge landslips are especially spectacular and clearly visible on mountainous terrain on all continents. Where a rocky upland is underlain by a weak zone in which pore fluid pressure exceeds lithostatic load pressure, the entire upland terrain may slip incrementally or catastrophically into an adjacent basin on a downward sloping surface that need be no more than 1 to 2 degrees. Today, terrain emergence has in many prominent places elevated and exposed these slip planes to view and erosion. Historically, geologists attributed these features to horizontal compression that created *thrust faults*. Other geologists have all along maintained that only gravitational sliding due to gravitational instability created by positive isostasy could explain the great *thrust faults*. Over-thrust faulting is best described as the natural consequence of off-loading after the development of isostatic imbalance and the intrinsic imbricate fault structures in the mountain fronts as the terminal features that develop where off-loaded blocks collide with barriers to further slippage.

The rise of mountains and the sinking of oceanic trenches, then, are explained by hydride theory without horizontal compressive forces in the crust or the invocations generally called plate tectonics (*i.e.* drifting continents, diving slabs, subduction of oceanic crust, and mantle plumes of viscous silicate rock). Hydride theory as creator of isostasy also creates the gravitational instabilities that lead to off-loading on overthrusts.

Continents, built over eons by multitudes of atoms of silicon transferred from the interior, are elegant signatures of Earth's past expansion. Their roots accrete mass as rising hydrides react with water that seeps downward in the fractures of the continental mass. The silicon substitutions for heavier metals create silicate minerals of less density and more volume than their predecessors, and isostasy causes the mass to rise as mountains. Where the process occurs in the upper mantle below the level where free water saturates the crust, hydrogen is released as the silanes give up their silicon. Whether net densification or de-densification occurs is a function of pressure and the amount of ambient hydrogen as well as the amount of silicon substitution and rock volume change following metal substitutions.

Where migration paths for ambient hydrides are blocked, the stage is set for violent breakouts and deep-seated earthquakes. This may occur beneath a continental platform, where earthquakes should not occur, according to plate tectonics theory. Rock volume increase and its de-densification, along with ongoing effusion of hydrides, explains these earthquakes. Rock volume increase and de-densification amount to the explanation for Earth expansion. Dense, energy-rich hydrides of the inner Earth combine with oxygen from surficial water or atmosphere and add bulk to Earth's surface. The mass of the Earth does not change, but is shifted outward generally, thus slowing the rotation and reorienting the axis. All of this arises from the evolution of hydrides from the Earth's interior.

**Summary**

The tripartite grand processes of Earth tectonics, volcanism, magmatism, and expansion of Earth's diameter require heat and reduced silicon that only effusing silanes can supply. The deposition of silicon-rich, relatively low-density minerals made out of former gases on and within the crust is the formula for Earth expansion—that continents grow above plumes of effusing hydrogen and hydrides rather than the imaginary rock plumes of the plate tectonics fanciers; that they rise isostatically and incrementally over geological time; that each increment of growth is followed by erosion and gravitational redistribution of excess mass, *off-loading*, in other words, of isostatically overbalanced, emergent rock into adjacent basins. These are the logical and inevitable consequences of an hydridic Earth core.

Hydrogen, *the first element*, through its hydride processes and its capacity for transporting mass and energy, directly carries silicon, *the third element* loaded with primary energy, from Earth's interior to crustal levels. The spoiler for silane transportation is oxygen, *the second element*, which waits at crustal and upper mantle levels to disrupt arising hydrides and provoke primary endogeny. Expansion of the solid Earth resulting from this kind of endogeny is, therefore, not more nor less than *primary tectonism*, whether incremental or catastrophic.

In the tectonic order, elements one, two, and three, (H, O, and Si) are the primary agents of change as well as the prime building blocks in the physical world. It is interesting to note that H, O, and C are the primary agents of change and the prime building blocks in the biological world. Thus, the main transition element in the physical world, Si, is the sister of transition element, C, of the biological world. Oxygen is the spoiler/moderator in both worlds; H is the driver and spontaneous organizer in both worlds.

In the two worlds, physical and biological, spontaneous organization imprints holograms on populations of atoms, the holograms organize atoms into molecules and the molecules into clusters, crystals, and catalysts. In the world of biology holograms create enormously complex molecules and whole organisms, life forms. All of these holograms in both worlds depend on the internal energy of hydrogen to carry on their self-replicating ability, primarily using H, O, and C in the biological world, and H, O, and Si in the physical world. The substantial internal energy resources of hydrogen, as demonstrated in the hydrogen bomb, is the original source of energy for spontaneous self-organization, for hologram coherence, and for catalysis in both worlds. Philosophically, the holograms can be

***Hydrogen continues on page 70***

New Energy Times Archive

# Is This Really True?

**Lev Sapogin***

I have written many times about the new sources of energy and even about the so-called "perpetuum mobile," however, today every child having as parents professional physicists, gets "genetically" the certainty that such things are absolutely impossible and they do not deserve any kind of discussion. However, what is worse, I continue to write about this offense for contemporary science, thus defying the godfathers of the commission for "pseudo-science" of the Russian Scientific Academy, led by the Academician E. Krugliakov (who is obviously envious of the laurels of Trofim Denisovich Lysenko, the famous scientific dictator of the Stalin era).

The facts confirming the existence of such new energy sources are accumulating, and when some critical mass is attained, the situation will suddenly change. Let's follow the facts in some order.

Last summer I was invited by Swiss physicists (the director of the Institute of New Energy Sources from Egerkinchen, Adolf Schneider) and visited some scientific research organizations. It is quite strange that small Switzerland has such an institute while the great Russia does not. The aim of this visit was not more and not less than to explain the function of a device which produces energy from nothing, *i.e.* a perpetuum mobile. The Swiss device is: Testatik Machine M/L Converter from the religious group Methernita and the address: Methernita, CH-3517, Linden, Switzerland, Phone: ++41 31 97 11 24.

I will tell what I know, heard, and saw. Such devices exist today in Switzerland in the small town of Linden, near Bern. A part of this town belongs to the religious Christian community, which is isolated and carefully protects itself. This community has about 250 members and many of them are physicists who graduated from the universities of Geneva, Laussane, and Bern. However, there are not only research laboratories; the community also owns a television center, movie studios, a small factory producing furniture, shops, garages, homes, and other services. And, as you surely have already guessed, this community does not use any electric power, and this fact is the most certain fact of this whole story—they do not pay at all to the local utility, as stated by meticulous journalists. Simply, one of their houses is equipped as a *sui generis* electrical station, and there they obtain the energy from nothing! The inventor of this valuable, inexhaustible source of direct current is the Swiss physicist Paul Baumann. He created his first experimental device in a Swiss prison when he was kept there some twenty years ago. I could not find out why and how he got in such a place not fit for creative activities, but perhaps I did not understand it well in English.

I have to tell you that this is not a unique case of great discoveries made by people in prison; and in this respect, Russia is the "leader." For example, the Russian mathematician M.V. Ostrogradski elaborated his theorem (now called the Gauss-Ostrogradski theorem) when he was in prison for a long time, at Sankt-Petersburg, due to a great debt at a game of cards which he was not able to pay. It is funny that in the same building on the Fontanka now is the University of Fine Mechanics and Optics of St. Petersburg. Even funnier is the following fact: he was able to send this theorem from prison to Paris, where at the French Academy Cauchy himself found it outstandingly interesting and had it published in the works of the Academy. Cauchy, in his letter of reply, expressed his wonder how such a highly qualified man could work in prison. When Ostrogradski explained that he was not working there but was in prison for a debt, Cauchy, who was rich, paid this debt.

Readers may doubt that such an action was possible at all in the case of a Soviet prison. Any scientist can remember how complicated and difficult it was then to send any letter to foreign countries or to publish any paper there even when the author was not in prison.

I will now shortly describe this fantastic device: there are four types (sizes) with powers of 0.1, 0.3, 3, and 10 kW. The device as such is similar to a standard electrostatic machine with Leyden jars, used for demonstrations. Inside are two acrylic disks, to which are glued thirty-six narrow sectors of aluminum foil; the disks are rotating in opposite directions. For the first devices the disks used were usual gramophone plates. The device is started by impulses applied by fingers, in opposite directions. The speed of rotation is 50-70 per minute. After these impulses, the disks spin freely and can easily be stopped with the fingers; a direct current voltage of 300-350 V is obtained, at 30 A. The mechanical energy consumed by rotation (according to the measurements of Marinov this is only 100 mW) is 100,000 times smaller than the electrical energy generated. The greatest device, at 10 kW, has plastic disks with a diameter of 2 m, the smallest 20 cm, and the whole device is not too heavy, *e.g.* the 3 kW machine weights approximately 20 kg.

The process of separating the charges (which needs a consumption of energy) hardly brakes the disks at all. By connecting the current to a lamp of 200 W, the speed of rotation does not change. No phenomena of heating or cooling of air during the long-term work takes place; only a weak smell of ozone is emitted. The system is noiseless, compact, absolutely clean ecologically, and can be placed everywhere.

The more curious or doubting reader can take a look on the Internet and find supplementary information at the following websites:

http://www2.murray.net.au/users/egel
http://members.tripod.com/~kilowattfree
http://www.geocities.com/ResearchTriangle/Lab/1135/testats.htm
http://colossus2.cvl.bcm.tmc.edu/!wje/free_energy/testatika.htm
e-mail: leo@zelator.in-berlin.de

The leadership of the community believes, perfectly correctly, that the broad spreading of such generators would cause a thermal explosion because all the energy used by Mankind goes to the energy "dump," that is, is transformed into heat and, eventually, this leads to the heating of the environment. They have absolutely no confidence (and that is not without basis) that people would be able to use this discovery wisely, and consider that the harm caused by this discovery could be even greater than that due to nuclear, bacteriological, or conventional weapons. Their main idea is that Mankind has to live in harmony with the environment and has to use completely the energy of wind, sun, water, and so on. For this reason, the achievement of this community is shrouded in secrecy. Access by people from outside is almost impossible, and this important discovery will not be donated to the people.

Further, a real detective story began here: Professor Stefan

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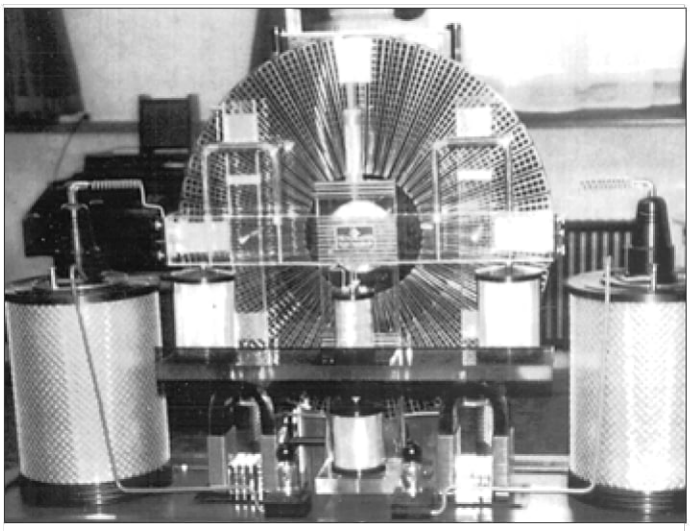

**The Testatik Machine**

Marinov, from the Institute for Fundamental Physics in Graz, visited this community two times (July 1988 and February-March 1989). He got a device with the power of 300 W at 0.3 A for study in his laboratory. As far I have understood, even the inventor of this device does not completely understand its working principle and, therefore, out of scientific curiosity he got in touch with Marinov.

In 1989, Professor Marinov published the book *The Thorny Path to Truth: Documents About the Destruction of the Conservation Laws* (International Publishers East West); it contains a lot of photographs, excerpts of measurements, description of the devices, and has organized the scientific group "Methernitha Group Stefan Marinov Free Energy." In this book there are interesting words:

> I can confirm, beyond the smallest doubt that this machine is a classical perpetuum mobile. After a first initial impulse it continues to move alone for long time and produces constantly electrical energy in a quantity of 100 Watts. . .In such a device, the motor and the generator, separating charges are bound together. . .However, till now it is not clear how this can happen. As far I know, nobody was able to build something similar in other places.

In 1995, I gave a paper, "About the Mechanism of Energy Generation in the Unitary Quantum Theory," at the International Cold Fusion Conference at Monte Carlo (ICCF-5) and another one, in 1996, in Japan about the "Generation of Energy at the Solutions of the Schrödinger Equation." After that, in April 1997, Marinov got my service telephone number and called me (he spoke Russian well). It is the first time that I heard the word Testatika, and I know about similar devices. We discussed my prospective visit to Graz, Austria and the Institute of Fundamental Physics. However, on July 15, 1997, Professor Stefan Marinov was thrown out the window of the town library of Graz by unknown people and died, taking many secrets with him. [Editor's Note: Marinov's death was officially declared a suicide, but some doubts remain. See *Infinite Energy*, 13/14, 83.] Any good detective might get the idea that the unknown people worked for the coal, oil, electricity, or gas companies, and the religious community has nothing to do with it; however, I have a constant aversion to detectives.

Our times are becoming more cruel and I want to explain this to the readers. It is well-known that the great Serbian, Nikola Tesla, searched for similar sources of energy in the U.S. When Morgan, who financed his project, was informed that Tesla was working on the new source and was very near to his aim, he immediately ceased the financing and prohibited the continuation of the work. Morgan considered that such a source of energy could render a great number of people independent and it would be difficult to lead them. Tesla came to his death, too.

In the dark Middle Ages such people were simply burned at the stake; in the era of socialism, from considerations of humanity, they had no chance to publish anything, and now, they are completely democratically thrown out the window of a library. This is some progress because a fast, sudden death is obviously better than being burned, as in the Middle Age tradition. Readers can speculate what the advantages of this may be, and for whom.

But let's go back to Egerkinchen. First, the Swiss authorities discussed with the community my visit, together with a well-known physicist from the Bern University. It was planned at the annual meeting of the absolvents of the Bern University. However, at the last moment I was not admitted to enter the community, but the physicist I knew spent a full day there. As a consolation, they took me for a trip in the mountains and, thus, I got a look at the community through the window of the car. Really, besides the houses and a big laboratory building with an inscription "Methernitha," there is only a wind turbine and a small water wheel. I think the power of these sources of energy could not be greater than 2 to 5 kW.

The next day I had a long discussion with my physicist colleague and learned many interesting details. When he came back from the community, he seemed to be shocked and intellectually confused. And I have to say that this Swiss physicist is


**About the Author**

Dr. Lev Sapogin (Ph.D., Nuclear Physics, 1966; Doctor of Sciences, Quantum Physics, 1971) worked for the ministry of Defense of the U.S.S.R. until 1972, after graduation in 1959 from the University of Radio Electronic Engineering; this included work in nuclear physics, radio physics, and quantum physics. He then worked in quantum physics at the Academy of Science of the U.S.S.R. from 1972 to 1984. From 1984 to the present, he has been the Physics department head at the Technical University. His research activities include the theory of unitary quantum field, cold nuclear fusion, and new sources of energy. Dr. Sapogin is married and has one daughter.

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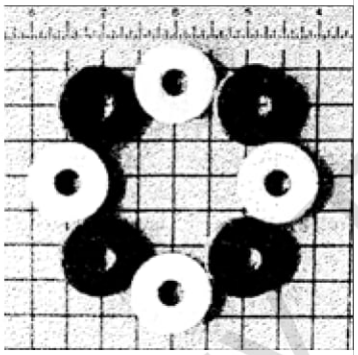
# Scanned Image of "Electron Gas"

**David L. Bergman***

**Reprinted with permission from *Common Sense Science*, February 1999**

Science *magazine printed a scanned image of electrons formed in the shape of a ring, but offered no explanation for the unexpected formation of electrons. The following letter to the editor of* Science *magazine was mailed to* Common Sense Science *in December 1998.*

Scanned images of current flowing through a two-dimensional "electron gas" (2DEG) show charge formed into many puzzling patterns.[1] "[T]hese patterns that are enormously sensitive to [magnetic] field[s]. . .range from long filaments to small droplets to large islands. . ." Researchers are in "shock" by these patterns that show electrons formed into "mysterious rings of compressible charge." "Now why would electrons form circles like that?"



Joseph Lucas found the answer to pattern formation by performing simple experiments with ring-shaped magnets that have a dipole moment similar to an electron's moment.[2] One pattern of magnets forms a perfect circle where an even number of adjacent magnets alternate pole directions. Other stable patterns of ring magnets form the "long filaments," "small droplets," and "large islands" observed by research teams.

His magnet experiments helped Lucas find an explanation to self-formation of electrons into the "enigmatic rings." The little-known *Lucas Model of Atomic and Nuclear Structure* also explains basic features of the *Periodic Table of the Elements*, *e.g.* the so-called "magic numbers" of electrons in atomic and nuclear shells and the limitation of only seven periods of stable elements. The model even predicts the correct spins of 1500 nuclides, whereas other better-known theories make wrong predictions for hundreds of the measured nuclide spins.

In other literature the 2DEG images are called "Quantum Dots," and research teams can only "speculate that. . .the laws of quantum mechanics. . .are responsible." But Quantum Theory insists that the electron is a point-particle—a feature that incorrectly predicts its spin and magnetic moment are zero—and assigns an intrinsic spin and moment to the electron. Quantum Dot images provide evidence of *structure* and *magnetic properties* of the electron, suggesting that quantum mechanics cannot predict the observed images without modifying fundamental tenets of Quantum Theory.

Lucas made predictions of the structural and magnetic characteristics of atoms on the basis of *physical models* published by A.L. Parson,[3] A.H. Compton,[4] S. Iida,[5] R.C. Jennison,[6] and D.L. Bergman and P. Wesley.[7]

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***Briefs continued from page 45***

exposure in reviews and other media. We reviewed it negatively in *IE*, No. 30, p. 44. This spring it appeared as an "alternate selection" of both the Library of Science Book Club and the Astronomy Book Club, and perhaps other related clubs. The "Arts and Ideas" section of *The New York Times*, April 29, 2000 (A15) featured a one-third page color photo of Park in his jogging attire, with the Washington Monument as background. This was part of a laudatory profile titled, "A Claim-Buster by Calling." Park's ideological soulmate James Randi is quoted, "You have to hose him down every two hours just to keep him cool." The article refers to his "litigious critics, who occasionally threaten to sue him for ruining multi-million dollar business prospects or for slandering them." *Voodoo Science* was, in fact, delayed a month or so due to the protestations of at least one person—most likely astronautical engineer Dr. Robert Zubrin, who rightly objected to being called by Park (in the galley draft) the leader of a "cult," the scientifically based Mars Society (www.marssociety.org). *Voodoo* was also favorably reviewed in the *NYT* Sunday Book Review section (June 4, 2000).

*Forbes Magazine* in its May 15, 2000 issue ran two adaptations from portions of Park's book: "The Alchemists of Energy" (p. 117) and "Voodoo Science and the Power-Line Panic" (p. 128). *Forbes* Editor William Baldwin promoted *Voodoo* in his "Side Lines" column (p. 20). He opined, "It has been known for more than a century that things like engines fueled by tap water are thermodynamically impossible." A letter of protest from yours truly was accepted for publication in a subsequent issue. *U.S. News and World Report* also carried an interview with Park.

The good news perhaps in all this publicity is that Park's foolishness and fraudulent statements about cold fusion, BlackLight Power, and so many other areas will eventually be known to many more people.


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*Triumph continued from page 24*

Institution in Washington, DC—a quaint addition to its collection of memorabilia about the discovery of nuclear reactions in the cold and the transmutation crisis at Texas A&M during the 1990s.

The process of academic freedom at Texas A&M University had been strained, some would say broken, by the suppression of Packham's research. Since the work had been published before the Ph.D. degree was resolved, it was in the public domain. Its suppression as a part of Packham's thesis reflects badly on Texas A&M, as do all the other outrages that we have reviewed. Perhaps in the fullness of time, Texas A&M will realize what happened on its campus and take steps to honor John Bockris and his students, rather than to revile them and devalue their historic work.

**Excusing Texas A&M?**

Since Bockris retired from Texas A&M in 1997, he has thought about the treatment meted out to him. He prepared this assessment for this article:

"The fact that the so-called cold fusion phenomena has been so much confirmed in various parts of the world (2,000 publications!) and that the American Nuclear Society has agreed for the last three years to host sessions on low-energy nuclear reactions, all shows that we were right in 1989 with the first scientific measurements of tritium, and again in 1992 with the first published measurements of transmutation among metals. I stopped counting at 174 papers with the tritium confirmation, because there seemed no point in obtaining further confirmation of our pioneering work. Tritium had its day when its finding was primary evidence for nuclear reactions in the cold, but now the barriers to analyzing helium have been overcome and Melvin Miles and Michael McKubre have shown that helium production is one of the main products, and accounts for most of the excess heat. The production of tritium is no longer of primary importance.

"How is it that a University can react so strongly against a Distinguished Professor who obtains new and unprecedented scientific research results? Is not a university the place for this kind of thing? Such fundamental new and disturbing results might never be tolerated in industrial labs. Further, heads of groups in government agencies are not pleased when something unexpected and fundamentally new is discovered, because it upsets their plans. Thus, where in the United States, is New Science to be created? Is it not in the universities? Do not the words 'academic freedom' mean quintessentially that a man or woman can research what he or she likes and publish results according to what is found? Isn't the fact that they are published in refereed journals sufficient for their intended integrity to be confirmed? All these questions are apparently answered in the negative for Texas A&M University, and this is a tragedy. However, I have tried to look at it from the point of view of the President's office.

"First of all, Texas A&M is without doubt, a football school. I mean nothing pejorative in this, but the fact is that when one speaks in Pittsburgh or Boston or Los Angeles about 'the Aggies,' they are not talking about the Distinguished Professors of the Physics or Chemistry Departments (nor even those in Agriculture); they are talking about the football team.

"One of the higher administration officials at Texas A&M has described to me just how strong the influence of the success of the football team is and what influence it has on the Board of Reagents. First of all, as in other universities, the coach of the teams is reported to receive an income larger than that of the President. The Board of Reagents is the controlling body of the University, and its degree of satisfaction is strongly influenced by the football team! When the Aggies win a game, the donations from rich people to the University increase. But when the Aggies lose, it declines!

"The second aspect of Texas A&M, which has affected what happened to me, I think, is the militaristic background. By now, only about six-percent of the student body are in the officers training corps, but it seems that the idea of 'command from the top' pervades the atmosphere at Texas A&M. Indeed this has come to the fore much more in recent years with a new president who seems to want to have a hand in 'controlling' everything and who has caused a decrease in the atmosphere of relaxation on the campus, which is so necessary to the prosecution of disinterested inquiry. A case in point: The recent persecution of a man in the computer sciences department, based upon the fact that he had used grant monies to support a course he was teaching extra-murally.

"Briefly, the kind of publications which a University of this kind likes are those which confirm the paradigm. Of course, the papers have to be original and have to constitute an advance, for example, as refereed papers published in the *Journal of the American Chemical Society*. These papers should be a little better than the papers which have been published there before. This will disturb nobody and also not make much difference, but it will not scare people and that leaves everyone smiling and happy.

"Texas A&M University, military history, concentration on football, should not, however, be criticized too severely for giving in to the requests of the professors who tried to harm me. In spite of all, the final results were favorable to me, the due academic process held, although I undoubtedly underwent two to three years of totally unjustified persecution. It is worth quoting the situation at Harvard when John Mack published a deep study of what a number of his patients related during hypnosis. The essence of Mack's book, *Abduction*, is to say that the persons concerned passed every test for sanity, but claimed they had been abducted and operated on in space vehicles to provide genetic material. Mack was duly investigated, as was I, but his trials were much shorter (three to four months) and the result more friendly and encouraging to the goal of basic scientific research, the establishment of the new."

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ysis or a mechanical stirrer. Fleischmann discussed a thermal pulse added while electrolysis is underway, when electrolysis bubbles ensure complete mixing. After the Q&A session, Fleischmann said to me that he does partially disagree with Storms, because he thinks that with proper cell geometry—with a tall, thin cell—mixing will not be an issue and a pulse of heat alone can be depended on. I took this comment to Storms, who said he actually agrees, but some people do use the wrong shaped cells. Fleischmann also pointed out that he measured temperature at two or more points in the cell to be sure there was mixing, and the difference in temperature was so small "you just shouldn't bother with it."

A "Wheatstone bridge" usually refers to a method of measuring electrical resistance, in which four resistors are connected. Three have known resistance, and the resistance of the forth is determined. Morrison refers to an analogous method to measure heat. Just after the Q&A session, Fleischmann explained to me that the main reason that method does not work well in these experiments is that the bath heaters produce a great deal of thermal noise. I wish he had taken a moment to say that in his response to Morrison.

There is no such thing as foolproof calorimetry. Morrison's advice to "do good experiments" is about as useful as the stock market tip "buy low, sell high." There is no method which cannot be screwed up. Fleischmann's goal, to measure low level heat of approximately 5%, is particularly difficult to achieve, no matter which method you choose. All methods require calibration. In principle, methods like flow calorimetry require no calibration, but in practice you must calibrate. This was a telling encounter between Fleischmann, an expert in calorimetry, and Morrison, who does not appear to understand the fundamentals of the subject.

Morrison was not impressed by McKubre's helium results because, as he said, this is the same old thing. He is correct in a sense: qualitatively it is the same old heat, helium, and still there are no neutrons. Most people find it more convincing when the same results are obtained year after year, with better instruments and an improved S/N ratio, but Morrison is holding out for a different set of results that will conform to conventional hot fusion theory. As Ed Wall points out, quantitatively the helium results are four orders of magnitude higher than before, which makes this the "same old thing," but more convincing. Morrison does not understand that, because he is strangely innumerate at times. (I think he honestly does not understand.) The tritium results are also the "same old thing," but again, they are thousands of times larger than before. In earlier conferences and papers, Morrison often said that cold fusion is caused by the "cigarette lighter effect," even though this effect is 3 to 6 orders of magnitude too small.

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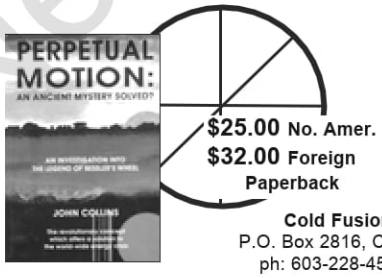


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plasma-electrolysis that he was doing work with underwater arc discharges that did not have the electrodes near the arcing! He accomplished this with a couple of clever methods. The first one was by means of an electrolyte diaphragm, which is a barrier between the cathode and anode. This barrier is an inverted ceramic "peppershaker" (called a "pepperpot" in the UK). The holes in the bottom are the only path for the current to flow through the diaphragm; under high voltage conditions, they act as high current conduits. The current density can get high enough for arcing to occur and we have experienced this in our initial attempts at replication. Mobberley prefers the use of a saturated LiOH solution and has reported preliminary significant overunity performance. He has a preferred means of accomplishing his "electrodeless arc plasma" by using a small pump to squirt electrolyte at one potential into a container with electrolyte at the other potential. I can only imagine what this looks like! He reports this gives him good control over the reaction by varying the pump speed. The terrific advantage of this method is that electrodes are not destroyed, as with the Mizuno-Ohmori tungsten cathodes, and so reactions can run for as long as desired, which makes heat measurement much more reliable and easy. Mobberley measures input power into an autotransformer, where it is still a clean 50 Hz sinusoid. The reader may remember that input power measurement into the Mizuno-Ohmori cell consists of measuring power directly into an arc, which has a virtually infinite frequency bandwidth, so is subject to considerable difficulties.

Mobberley also has reported success using, as a cathode, an ordinary spark plug with the ground lead removed. It seems that the alloy used in the center pin can stand up to the conditions of arc discharge for extended periods. Also, the ceramic mounting of the pin is ideally suited for holding the center pin without getting destroyed or allowing the arc to break the surface of the electrolyte, which could ignite the electrolysis gasses. We hope to have more to report on this exciting work in a future issue. Stay tuned.

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Editorial continued from page 5

We at *Infinite Energy* and New Energy Research Laboratory seem to be one of the only groups interested in developing demonstration cells so that the tangible embodiment of the excess heat phenomenon can be reliably seen and studied by greater numbers of laboratories, individuals, and industries. A discussion we had at ICCF8 with one of the leaders in a now scaled-back cold fusion company proved to us there is little understanding in the field about the critical need for such a direction.

The cold fusion field is similarly befuddled with actions by good friends that are totally counterproductive and incomprehensible, but all too familiar. For example, we learned that several excellent journalists had been turned away from ICCF8 by the conference leadership, which wanted to be "selective" in what journalists were allowed to attend! This is part of the historic devaluation of efforts to develop good "PR" for the cold fusion community. And, there remains a significant trend within the cold fusion field to maintain secrecy, so that the meager funding that may be available is retained by small groups of "insiders." It is not uncommon to hear cold fusion theorists and experimenters worry that the outside world's impression of the field will turn around too quickly, many others will begin working in cold fusion, and they will be left without credit for their many years of work and sacrifice.

But some *have* gotten the right message. We very much like what Professor Akito Takahashi of Osaka University had to say in his summary of ICCF8: "Science only is not enough." He said there is a need to interest "industry people, people from other fields, and young people" in cold fusion. We couldn't agree more!

**Moving On. . .**

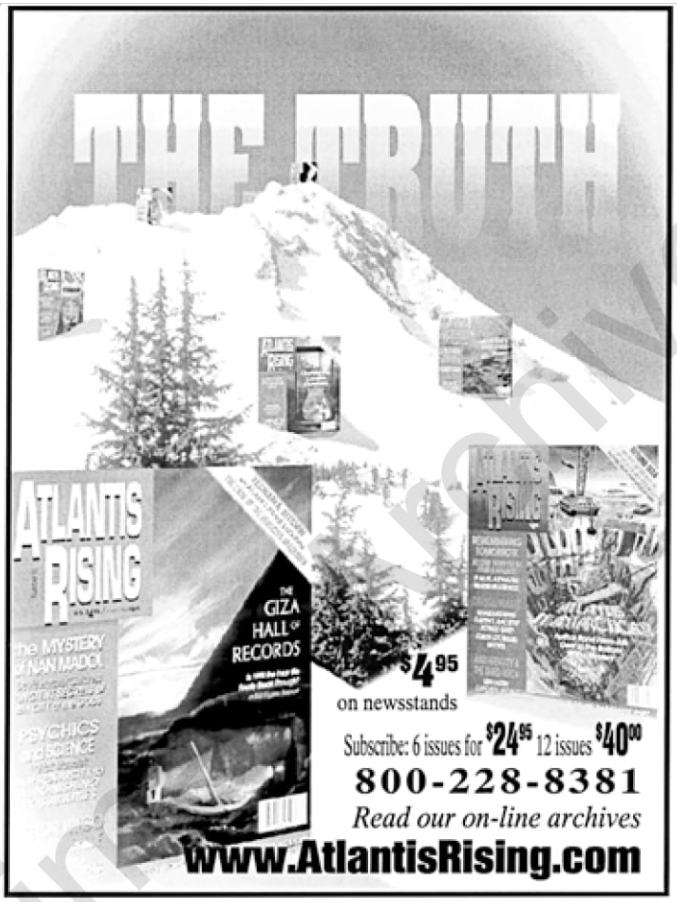
Our former Associate Editor, Jeff Kooistra, is no longer with *Infinite Energy* after a little over a year of good service on our pages. Others will continue to provide expert editorial discussions in the electromagnetics arena, which was Jeff's forté. He has moved on to other pursuits in the field of frontier physics and we wish him well. He'll likely continue working with a provocative theorist, whom I had met long before Jeff did (though it was Jeff who pro-actively grappled with his theories), a man from academia with military-industrial connections who is a deep scholar of physics and more.

Jeff's mentor insisted on remaining anonymous, and that became a great problem for a magazine that prides itself on openness. "Mr. X" believes he has a solid handle on the direction that electromagnetic and gravity theory must take if the world is to benefit from what may well turn out to be very productive insights. More power to him. We wish him well too.

Regrettably, Mr. X felt he had to be highly secretive. His desire for unwarranted control over communications and editorial policy, led to severance of both "Mr. X" and Jeff from Cold Fusion Technology and *Infinite Energy*. It was a very difficult decision for me to make, but circumstances dictated that action. Fortunately, we are the stronger for it. The technical insights that Jeff began here will continue to be developed at our New Energy Research Laboratory (NERL). They will eventually be demonstrated to the world, as we had hoped to have done earlier. ◆◆◆

The Irish physicist, George Fitzgerald (1851-1901), claimed that producing lightlike waves by oscillating electric forces was impossible, yet not many years after publication of his paper, the feat was accomplished by Heinrich Hertz (1857-1894).

Submitted by David Moon

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### About the Author

Edmund Storms obtained a Ph.D. in radiochemistry from Washington University (St. Louis) and is retired from the Los Alamos National Laboratory after thirty-four years of service. His work there involved basic research in the field of high temperature chemistry as applied to materials used in nuclear power and propulsion reactors, including studies of the "cold fusion" effect. Over seventy reviewed publications and monographs resulted from this work as well as several books, all describing an assortment of material properties. He presently lives in Santa Fe, New Mexico, where he is investigating the "cold fusion" effect in his own laboratory. These studies have resulted in fifteen presentations to various conferences, including the ACS and APS. In addition, twenty-one papers have been published, including three complete scientific reviews of the field, one published in 1991, another in 1996, and the latest one in 1998. In May 1993, he was invited to testify before a congressional committee about the 'cold fusion' effect.

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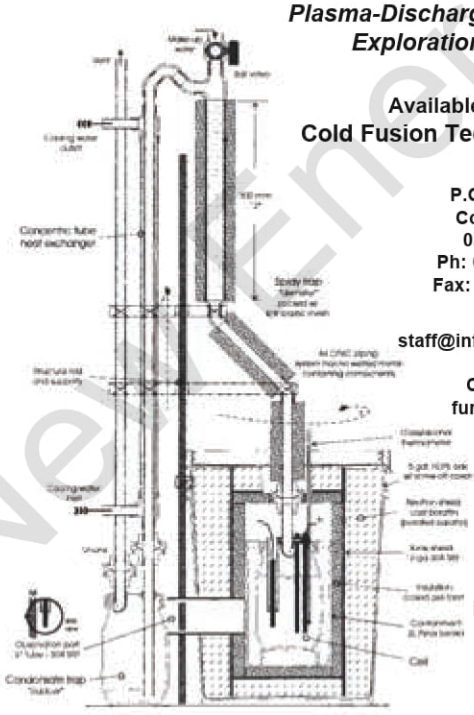

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thought of as expressions of memory and spontaneous organization as intrinsic will. Hydrogen, then, can be said to be the provider of the spark of life and the dynamo of the planet.

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### About the Author

Charles Warren Hunt, III graduated with a B.S. degree in Geology from the California Institute of Technology in 1945. After nine months with Standard Oil Company of California, Hunt went to work with a small, very successful exploration company, and after two years with them, set out on a consulting career in oil, gas, and mineral exploration, which he has maintained ever since. He worked in the Mediterranean in 1948 and relocated to Alberta in 1949. In 1951, Hunt was the discoverer of several major Alberta oil fields (Joarcam, Hamilton Lake, Baxter Lake). In 1957, his work in the foothills resulted in the first very high (93% $H_2S$) sulphur gas discovery in Alberta. His work in minerals developed the first high-purity silica mine in western Canada (at Golden, B.C.), production of which commenced in 1978. In 1974, he opened a gold mine with his son in Carlin, Nevada, the Goldstrike Mine, now the largest gold mine in North America. In 1986, Hunt undertook the writing of a book on catastrophic geological processes. In the course of research on it he recognized that hydrogen gas, hydrocarbons, and metal hydrides must emanate in large volumes from the Earth's mantle. His book, *Environment of Violence*, was published in 1990 by his self-publishing company, Polar Publishing. A sequel, *Expanding Geospheres*, followed in 1992. Hunt is also author of several papers. To test his theories regarding petroleum accumulations in granite host rocks, Hunt formed the Anhydride Oil Corporation to drill at Ft. McMurray, Alberta. In 1994, despite widespread skepticism, a 1650 meter well was drilled. Testing was not completed due to inadequate funding, but Hunt continues to run the needed tests, confident that his theory is right and will soon gain recognition.

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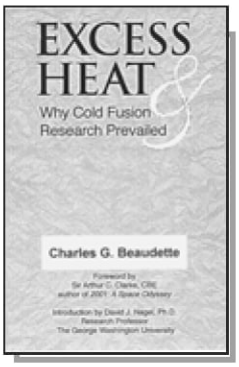


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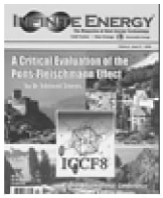


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